
MEDICAL REPOSITORY,

FOR

FEBRUARY, MARCH, AND APRIL, 1810.

CASE of a wound in the POSTERIOR TIBIAL ARTERY; with some remarks on the natural and artificial methods of stopping HÆMORRHAGES. By THOMAS HUBBARD, M. D. Pomfret, (Conn.)

A. W. of B——, aged 10 years, received a wound by an axe, just below the ankle-joint.—The hæmorrhage was profuse. A physician was called, who, by means of lint rolled in flour, compression, &c. suppressed it for the time. The hæmorrhage however, continued to recur at intervals the night following and the next day, and was restrained by similar measures. In about a week afterwards, the hæmorrhage again took place, and was restrained as before. It recurred at intervals of a few days till about three weeks after the accident, when I was called to visit him.

March 25, 1809.—A considerable hæmorrhage had taken place the day before, as I was informed; but upon removing the dressings when I visited him, no discharge of blood followed. There was a transverse wound a little below the malleolus internus. The wound appeared originally to have been about one and a half inch in length: the angles, and about one half its length was covered with a cicatrix. The sore was filled with a substance like coagulated blood, in which there was a strong pulsation.

Having applied my fingers above the wound between the tibia and tendo achillis, I found I could with ease stop the pulsation in the wound. This also demonstrated, that it was the posterior tibial artery that was wounded.

As there was at this time no hæmorrhage, I did not use the ligature, but concluded to try the effect of compression in the case. After drawing the edges of the wound together with adhesive plaister, I applied a compress three in-

ches long, very narrow and thick, in the course of the artery above the wound : over this were applied other compresses, and a pretty tight bandage.

On visiting him again three days afterwards, I found less pulsation in the wound, which had advanced in healing. The dressings were renewed at intervals of about three days, as the discharge was small.

About a week after my first visit to him, the artery again burst open, and a spoonful or two of blood was discharged. I had previously instructed them to compress with the fingers above the wound if the hæmorrhage should return; in which case I was to be notified. However, as the discharge of blood was trifling, and as I lived about 10 miles distant, I was not informed of it till two or three days afterwards, when on visiting him again, I found him much as before : the quantity of pus discharged from the sore was, however, rather increased.

I again hesitated about securing the artery with the ligature, and decided to try further the effect of compression, which was employed as before.

April 13.—I found the sore healed, except a small spot in the middle, which looked like dried coagulated blood. There was but very little discharge from it, and the pulsation had almost disappeared. The same method continued, and left the case to the care of the family. They agreed to notify me if the hæmorrhage should return.

April 23.—I was sent for in haste, as the artery had again opened, and a considerable discharge of blood had supervened before the hæmorrhage could be suppressed. It had however ceased before my arrival, but I concluded no longer to postpone the only effectual remedy. It was evening, but I proceeded immediately to the operation.

An assistant having made compression with his fingers on the artery above the wound, I made a longitudinal incision in the course of the artery through the original wound—beginning one and a half inch above, and ending half an inch below it. By cautious dissection I discovered the artery, in which was a longitudinal wound about a quarter of an inch long. I passed a common crooked needle armed with a ligature, with the eye foremost, (as I had no blunt needle at hand) around the artery one third of an inch above the wound, and tied it. The compression above was then removed, and as no hæmorrhage followed, the edges of the wound were drawn together by strips of adhesive

plaister, over which were applied compress and a bandage. The original wound at the time of the operation was all healed, except a small sinus communicating with the wounded artery. No hæmorrhage afterwards supervened, and the wound healed rapidly.

We see a striking analogy between this case and aneurism. I some years since operated with success for an aneurism of the humeral artery, occasioned by blood-letting. The discharge of blood externally, in that case, was stopped by lint, compression, &c. A small tumour with pulsation afterwards arose in the manner described by Mr. B. Bell. This increased not regularly, but at intervals. There would for instance be an increase of one half in the tumour in a few minutes; then for several days it would remain stationary. I was first called about three weeks after the accident. The tumour was at that time large, and while I was preparing to perform the operation, there was an increase in its size of nearly one half in a few minutes. The tumour extended almost to the axilla. On operating I found part of its contents fluid blood, probably just escaped from the artery. The increase of size in the aneurism answered to the hæmorrhage in the other case.

I was called a few years since, in the case of a wound of the anterior tibial artery, between the bones of the leg, about mid-way between the knee and the ankle-joint. It was about a week after the accident. There had been repeated returns of hæmorrhage preceded, as I was informed, in every instance by visible and strong pulsation in the coagulated blood, with which the wound had been filled after the first hæmorrhage. From what happened in this case, I would caution the young surgeon against passing a ligature around an artery in an old wound. After cleansing the wound of coagulated blood, I used the ligature without enlarging the wound, as it was originally large. Though I have no doubt that the artery was really included in the ligature, as the hæmorrhage instantly stopped, yet in about a week afterwards the hæmorrhage returned.

I then enlarged the wound upwards, and with a large crooked needle, armed with a ligature, introduced as deep as possible, and passed obliquely from one bone to the other, I secured the artery, and the patient recovered.

From these facts it appears, that in partial divisions of the arteries, when the hæmorrhage is restrained by any means except such as entirely obliterate the canal, a sac is

formed of the coagulable part of the blood, which resists the force of the artery for a time. This sac is liable to be ruptured, and if I may use the expression, healed again; or a new sac formed.

We are told by writers on surgery, that hæmorrhages from wounds are frequently preceded by a throbbing in the part. This is probably occasioned by such a sac as has been mentioned. There can be no reason for supposing that the action of the wounded artery itself, would be sufficient to produce this symptom. Wounds from which considerable hæmorrhages occur, are not often attended by much tension or inflammation.

When considerable hæmorrhages from wounds recur repeatedly, I think it may be laid down as an axiom in surgery, to use the ligature when it is practicable. The sooner this is done the better, as it has been the opinion of eminent surgeons that this remedy sometimes fails, either from the reduced condition of the patient after hæmorrhages, or more probably from some diseased condition in the artery itself.

OPINIONS and PRACTICE of JOEL ABBOT, *Physician at Washington, Wilkes County, Georgia, concerning the effects of ALKALIES, to prevent the VENEREAL DISEASE: Communicated to Dr. Mitchill.*

“DEAR SIR,

I HAVE considered it a good maxim, that every man should some way or other leave the world bettered by his life; and having been a number of years in the practice of medicine, I am disposed to throw my mite into the scale of medical science, in opposition to the weight of human maladies.

I feel the more induced to make the following communication to you, on account of having derived from your writings, in the Medical Repository, the ideas which led me to the opinion that the melancholy consequences of the venereal disease might be certainly prevented, by a little timely care in the use of alkalies.

In large cities, where municipalities have thought it wiser to wink at the assemblage of unchaste women, than suffer youngmen of impetuous passions to commit greater mischief;

the terrible consequences of pox, gonorrhœa, and mercury on their constitutions, will ever be beheld with sorrow and pity by benevolent minds, till something can be instituted as a preventive of those destructive maladies. And we can have no hope that young men will lose the desire, or acquire the prudence of passing by the invitations of women of ill fame, so long as the exciting power of a *peculiar secretion* has its wonted action on the vigorous and healthy. They ought therefore to be "saved from themselves."

But should there be found an easy, safe, and certain preventive of the venereal disease, used by all who expose themselves to its virus, for a few years, it would tend to root out the disease from amongst us. Such a desideratum is worthy of the attention and consideration of the faculty. In this regard, I feel assured our object is found in alkalies; and that alkaline substances arrest, decompose, and completely destroy the venereal virus, by coming in contact with it.

Two drachms of salt of tartar, in seven or eight ounces of water, have, in every instance where I have known it used, prevented the venereal disease, by washing the penis immediately after impure coition. In ten instances I have given this alkaline wash to young men who cohabited with women I knew to be infected; some with the pox, and others with the gonorrhœa.—They used it, and all escaped infection. I have good authority, that fifty others have used the same wash in similar cases without a single failure of success. A waggoner had to convey in his waggon, eighty miles, a young woman of bad character, whom I knew to have confirmed lues venerea. He procured from me the above alkaline solution, with directions, and repeatedly exposed himself to the danger of contracting infection, to the end of his journey; but cautiously washed and kept clean as I directed him. Six weeks afterwards he had experienced not a symptom of disease.

The knowledge of this efficacious preventive of the venereal disease, is derived from the splendid discoveries in chemistry at the close of the eighteenth century, which have shed distinguished light on almost every branch of physical science. It arrests, decomposes, and renders perfectly inert the venereal virus, by pursuing the following DIRECTIONS:—

As soon as the act of impure coition is over, let the whole penis be wet with some of the liquid, and especially

about the susceptible part of its extremity. Discharge urine as soon as possible, and with the urine, or some water, wash off all mucous or seminal matter that may have adhered to it. After this, wet the whole member again with this salutary wash, and perfect safety will be found from all the hidden stings of venereal infection. The reason why this salutary liquid is not recommended by certificates from distinguished personages, may be readily conceived. Delicacy forbids the measure, and it would savour of quackery. But many now have rosy health, and a sound system, who without it would have suffered the inroads made by Gonorrhœa, Pox, and Mercury, on their throat, nose, teeth, shin bones, and genital organs.

It is by no means intended by this application to encourage illicit connections, by affording safety to impure embraces ; for both religion and sound morality forbid an intercourse with harlots, "*whose feet,*" as Solomon says, "*go down to death, and whose steps take hold on hell.*"—Yet as criminal indulgences cannot (as would be wished) be altogether prevented, this preparation is calculated to guard against one of their most direful consequences ; and it is believed that as the vaccine inoculation will eventually exterminate the small pox, so this application may be a means of eradicating a loathsome complaint, attacking individuals at the very fountain of life and virility.

The opinion I entertain of the efficacy of alkalies in preventing the venereal disease, arises from the following argument :

The virus of the venereal disease has the character of animal poisons. Animal poisons probably possess an acidity which gives them their deleterious tendency. The venereal virus is then an acid, alkalies neutralize and destroy all acids ; therefore alkalies will destroy the venereal virus.

How far this argument will be looked on as conclusive, will be best determined by experience. The subject of poisons is difficult to investigate, and it is not my object to pursue it. That they abound with an acid, we have good reason to believe. That there is an acid quality in the venereal virus, I have no substantial proof ; and therefore claim only the weight of rational hypothesis for its support, till experience shall bear a better witness. Yet I have so much reliance on the power of alkalies to neutralize the poison of venereal infection, that I should be wanting on the score of humanity in not proposing to apothecaries and venders of medicine, to

offer it as a safe preventative of infection to all who would be induced to run the hazard without it.

You will please to dispose of this letter according to your opinion of its merit.

I am, Dear Sir,
With sentiments of great esteem,
Your Obt. Humble Servant,
JOEL ABBOT.

FACTS illustrating a DISEASE peculiar to the female children of Negro Slaves: and OBSERVATIONS, showing that a white woman by intercourse with a white man and a negro, may conceive twins, one of which shall be white, and the other a mulatto; and that, vice versa, a black woman by intercourse with a negro and a white man, may conceive twins, one of which shall be a negro and the other a mulatto. By JOHN ARCHER, M. D. of Harford County, (Md.) in a communication to Dr. Mitchell, dated Jan. 7, 1809.

SIR,

IN my Letter to you shortly after the meeting of Congress I signified that I should, in the course of the winter, transmit to you two cases in midwifery, that occurred in the course of practice. They are rare and uncommon; as such I thought they merited communication.

The first case was in 1783, and the second in 1795. The first was a negro woman, who belonged to Mr. W. M. about 39 years of age, who had been married to her first husband fifteen years, who left her, and declared she was no woman. After sometime she married another negro man, and in a few years she became pregnant. When she was seized with labour pains a midwife was sent for; when she came and attempted to assist her, she immediately informed her mistress that there was no passage, therefore the child could not be born. I was immediately sent for; when I came, and enquired of the midwife the situation of the fœtus, she said she could not tell, for there was no passage by which she could examine, or the child be born. I immediately examined, and found the statement of the midwife correct, and on inspection of the part, I found that the labia interna were firmly united, so as to close the vagina nearly, except a small aperture near the perinæum between one eighth and one

fourth of an inch diameter. At this aperture was the head of the child presented, and some of its hair protruded through the aperture. I immediately introduced a director between the united labia and os pubis, and with a crooked bistoury divided the labia interna completely, so as to open the passage into the vagina. The head of the child immediately progressed, and was born without any difficulty in a very short time. She soon recovered, and I never heard any more complaint, nor do I know whether she had any more children.

The second case was a young negro girl, belonging to Mrs. M'A. This case was similar to the foregoing: the vagina was nearly closed by the adhesion of the labia interna. The midwife, when she came, on examination, immediately told the girl's mistress the child could not be born without further assistance, as there was no passage for the child. I was immediately sent for. When I came, the midwife told me nearly as stated in the first case, and on examination I found her statement correct, but perceived the adhesion was not so firm and hard as the foregoing case, and doubt not, if the head of the child had advanced to the part, that the labia would have separated by distention of the parts by the head of the child pressing thereon as it advanced; for I separated the labia without the use of a knife by only forcible pressure. When this impediment was removed, labour progressed as is common, and the child was soon born. I here would remark, that negro female children are more liable to have an inflammation of the internal labia than white children; not because they are blacks, but from the occupation of the mothers, who have not time from their daily labour to attend their children and keep them clean by frequently washing. It is customary for negroes, who work out, that is, the field negro women, to take their children with them, when they go out to work, and take another small negro with them to stay with the infant to take care of it. When the infant is able to sit up, after the mother has suckled it, she sets it down on some old cloth or petticoat at the end of the corn row, where it sits until she hoes two rows, during which time it wets itself, &c. and sits thereon until the mother has hoed a row out and another back again. This being constantly repeated through the day, the slime becomes so acrid that the labia interna and contiguous parts become inflamed, and being kept constantly in contact, they unite and

from frequent inflammations, are rendered thick and callous. Female infants, that are just able to sit up about the time of planting corn, are those who are most liable to have the labia interna united by inflammation. These two preceding cases I have conceived worthy of communication. I therefore will thereon remark that the semen virile could only be transmitted through that aperture into the vagina, as the penis could not have entered at so small an aperture, and that there must have been other concurring circumstances to effect impregnation, such as spasmodic action of the uterus and tubæ fallopianæ. By this spasmodic action of the uterus, the semen virile is received or drawn into the uterus, and by the spasmodic action of the tubæ fallopianæ, &c. the ovaria are embraced, and an ovum conveyed by the tub. fall. into the uterus, where it meets with the semen virile and is fecundated. This is the usual and common process of nature; and not, as is conjectured, that the semen virile is emitted into the uterus, and conveyed by the tub. fall. to the ovarium, and there impregnates an ovum, which is thereby disengaged from the ovarium, and taken up by the tub. fall. and conveyed to the uterus as the nidus, where it is fostered until it comes to perfection or maturity; that is, that state of maturity which by its motions stimulates the uterus to contraction,* by which the os tincæ is dilated and the fœtus pressed forward into the vagina, and from thence ushered into the world. An impregnated ovum being found in one of the tub. fall. or an extra-uterine fœtus in the abdomen are no proofs positive of this last position, i. e. that the semen virile must be conveyed to the ovum, and the ovum impregnated before it can be disengaged from the ovarium, so as to be received by the tub. fall. and conveyed to the uterus. This process, in my opinion, is contrary to the natural action of the tubes and their universal formation. These situations of the impregnated ovum must have proceeded from too violent and forcible a contraction of the

* There is a certain period of animal gestation. It is not thereby meant that parturition solely depends on the motions of the fœtus to stimulate the uterus to those contractions that bring on what is called labour, but only that the motions of the fœtus, when the period of gestation is terminated, aid in exciting that contraction of the uterus that is called labour. Labour comes on with a dead child, as well as a living one, at the appointed time; but even a dead child, by its weight and pressure on the os tincæ, stimulates the uterus to contract and produce pains that are justly called labour; therefore, a gentle distention of the os tincæ will hasten pains that are weak, to a stronger and more frequent action.

uterus, which forced the semen virile into the tub. fall. and through them to the ovaria. This occurrence rarely happens, and therefore there are but rare instances of extra-uterine fœtuses. I have not known or met with but one instance in more than forty years. The diameters of the tubæ fallopianæ are smallest where they emerge from the uterus, and gradually increase their diameters to their extremities where they embrace the ovaria. This shape of the tubes contributes to the progress of the ovum from the ovaria to the uterus, as the ovum is attracted by the sides of the tubes, and as the diameters lessen, the attraction increases, and thereby hastens the progress of the ovum to the uterus, the rendezvous where the semen virile and ovum meet, and the ovum is fecundated. This appears to me the most rational and philosophical method to account for the impregnation of a female. These observations occurred to me on considering how impregnation could take place in the cases before stated. I therefore concluded that a mutual spasmodic action in both male and female is necessary: In the male, that he may emit semen into the vagina, and in the female, that the uterus may dilate to receive or draw into its cavity the semen deposited in the vagina. This spasmodic action, I conceive, is produced by the glans penis, stimulating by friction the glans clitoridis, and the glans clitoridis stimulating the glans penis until the spasmodic action is mutually produced, which occasions emission in one, and a spasmodic action of the uterus and fallopian tubes in the other. I conceive it absolutely necessary, to produce impregnation, that this spasmodic action should take place while the semen virile is in the vagina, and as absolutely necessary, that an ovum be conveyed from the ovarium into the uterus, while the semen is in the cavity of the uterus. I will state a case to you in corroboration of this opinion. A poor man and his wife lived in B—— County, near the public road. In the morning, the husband, after cohabiting with his wife, got up and went out to his daily labour, and left his wife in bed. Shortly after he went out, and before his wife got up, a negro man came into the cabin in which the woman lay in bed, and was prompted to have intercourse with her; he accordingly obtained permission, and cohabited with her also. The woman by these cohabitations was impregnated, and in process of time was delivered of two children, one white and the other a mulatto. In this case, I think the husband left semen in the vagina, but

had not excited the spasmodic action of the uterus. The negro afterwards, in cohabiting with her, excited that spasmodic action in the uterus, &c. which received the semen of the white and negro man into the uterus, and at the same time brought two ova from the ovaria which were there impregnated: the one was accidentally impregnated with semen of the white man, and the other impregnated with the semen of the black man, and therefore one was white and the other a mulatto. This last case I knew only by information from their neighbours, who often saw the children. I could relate another similar to the above, in which a white man cohabited with a negro woman after her husband, and the negro woman brought a black child and a mulatto at a birth.

These observations have occurred to me, and I conceived it might be useful to communicate them. They are at your disposal either to suppress them in toto or in part, or send them into the world. It is an attempt to account for one of the wonderful operations of nature, the propagation of the human species.

ADVANTAGES of ALKALINE INJECTIONS in VENEREAL AFFECTIONS of the URETHRA. *Communicated by Dr. G. WILLIAMSON, of Baltimore, (Md.) in a letter to Dr. MITCHILL, dated 10th August, 1809.*

SIR,

I AM happy to have it in my power to give you a new instance of the most salutary effect of alkali, as a *local* application. You, who have done so much for this valuable order of medicines, are well acquainted with their beneficial effects in gonorrhœa.* The case which I am about to give you is one of the most extraordinary in some respects that I have ever met with; although many of the illicit votaries of Venus apply to me for medical aid. The subject of this case was five or six months under the care of an

* I allude to your own remarks on the curative effects of the fixed alkali in Chancres, in *Med. Repos. Hex.* 1. vol. 2. p. 217: Mr. Hayes's relation of the cases of Gonorrhœa cured by soap injection in the New-York Hospital, (*ib.*) vol. 3. p. 302: The experiments of Messrs. Kunze, Torbert, and Brouwer, in the same place, on the acidity of morbid pus, (*ib.*) vol. 4. p. 297: and your collected facts on the advantages of alkaline applications in Surgery, (*ib.*) p. 149.

eminent physician of the place where he resided previous to coming here. However, when he came to this place, two months ago or more, the disease still continued; but so far from putting on any of the appearances of a gleet, which it almost always does after running this length of time, it still exhibited all the appearances of a recent and virulent gonorrhœa. My first application was, as usual, an injection of sacch. sat. gum arab. and water, in a few days succeeded by that of sulph. zinc. et acet. lead, &c. After using this for about three weeks, he grew much better; but from unguardedly exerting himself in a heavy lift, he brought on a discharge of blood from the urethra, accompanied with severe ardor urinæ, and other distressing symptoms; and from this time the gonorrhœa became worse, all our efforts to the contrary notwithstanding. He was bled, made daily use of the acidulous tartrate of potash in cold water as drink; took other cooling medicines, drank demulcents, &c. All warm drinks, or any thing that had a tendency to act as an excitant to the general system, or locally, was guarded against. Cold local applications were also had recourse to. These general remedies, with an injection of sulph. zinc. acet. lead, gum arab. and water were persevered in for some weeks, with the most rigid attention to diet, drinks, exercise, &c. and he would sometimes appear better, then worse; sometimes the pain would be but slight, then excruciating; sometimes the genuine gonorrhœal discharge would nearly cease, and then issue forth most abundantly; sometimes there would be no discharge of blood for several days, and then it would break forth anew; and such was the progress of the disease for several weeks, until the young man began to despair, and to be cast down with the fearful forebodings of the wretched life that he was about to be destined to; and I was not a little confounded at my want of success with a remedy which is generally so successful, and at the untoward symptoms which the disease manifested. However, I resolved to try the muriat of Mercury (corros. subl.); but so far from getting better by this application, every symptom grew worse, the discharge increased; he also bled more copiously; a chordee, which had occasionally been distressing for the several last weeks, now became increasingly so: at this time a diarrhœa came on, which exceeded all the other symptoms: we had for some time been under the necessity of giving opium at bed time: for opium alone, I now substituted

tinct. opii. balsam. capiv. gum arab. and water, and continued the corros. subl. injection; but the symptoms still increased, and in addition to the others, it was now to be feared that there was a stricture in the urethra: several symptoms indicated such a disease. I had suspected that the urethra of this young man was more than ordinarily irritable, and doubted the propriety of stimulating injections; and yet, as the disease was of so long standing I was at a loss what other course to pursue: however, such was the nature of the case now, that it became indispensably necessary to abandon all stimulating injections; instead of which, that of linseed infusion was used—also one prepared of about one drachm of salt tart. half an ounce of olive oil, a few drops of laudanum, and six ounces of water. These were recommended for the purpose of allaying the irritation and giving the mouths of the little blood vessels, that it was supposed had been ruptured, time to heal, and prepare for the use of the bougie which was now thought necessary to remove the supposed *tumours* or strictures of the urethra. After using these injections for forty-eight hours, my patient was much better, the blood ceased, the pain was much less severe, the stream of water was increased to near its natural size, and the chordee less troublesome. The next quantity was prepared without laudanum, and in about a week every symptom of disease had ceased. I have no doubt but this young man guarded against every connection with women, or any thing else which he had been told, or could imagine would injure him, for he was much alarmed, and I was very precise in my directions. I believe he occasionally used his injections too strong, and thereby injured himself. I have frequently used this alkaline injection in recent cases of clap, but did not suppose it was calculated for cases protracted like this. When I say we laid by the use of the stimulating injections and substituted the alkaline, I should be sorry to be thought so ignorant as not to know this is also stimulating; but it is much less so than that of the corros. subl. and yet it is necessary to guard against using even this too strong.

If it were not supposed by those who believe that the gonorrhœa and lues are two different diseases, that they can both be contracted at the same time, I could give you the most decided case to the contrary, which lately occurred in my practice, that I have ever seen or heard of; but as such a

principle is contended for, I do not suppose it necessary to intrude on you any remarks on this occasion.

LANCISI'S *Work on the NOXIOUS EXHALATIONS of MARSHES*: Published in Latin at Rome about one hundred years ago, and translated into English by SAMUEL L. MITCHILL, at Washington, during the second session of the ninth Congress, 1806—7.—Announced in our 2d Hexade, vol. 4. p. 304. (Continued from p. 245.)

CHAPTER TENTH.

The opinions of the moderns concerning these effluvia.

I. **A**LTHOUGH the moderns have differed considerably from the ancients in explaining the nature of marshy exhalations, yet several of them are so much alike, that they seem to have revived the notions of their predecessors: on which account the remark made by Terence in his Eunuch is very applicable to them, *that there is nothing said which had not been said before.*

II. Athanasius Kircher, in his *Scrutiny of Plague*, renewed the opinion of Varro: and when he recounts the causes of this woful malady, he mentions *septic exhalations breathing from those places which abound with the filthy mud of marshes and pools.* He affirms that these vapours are nothing more than certain aerial particles of a mixt, of the same property and nature with the whole; that they are destitute of life, but soon change to innumerable insensible worms; so that as many corpuscles as there are in the effluvia, as many vermicles will be hatched from them. These animated effluvia emit the more dangerous poison, in proportion to the greater vigour and energy they possess.

III. Christian Langius is a strenuous supporter of this hypothesis; for he imagined that poisonous spirits, or verminous atoms, were exhaled from putrefying bodies; and these, inhaled by the breath, or absorbed through the pores of the skin, scattered their venom through the bowels: but Mungetus, taking a middle course, avows his belief that these sorts of insects are not the causes, but the effects, of every thing whatsoever that corrupts.

IV. Others again, have revived the opinion of Vitruvius, by describing the marshy exhalations as noxious indeed, but, at the same time, destitute of vitality. Among those of great note is Joseph Quercetanus, who, as he thought Avicenna's opinion about the rotting of flax under water being one of the causes of pestilential distempers, was worthy of adoption, so he hesitates not to assert that the same Avicenna took no notice of the degrees and divisions of putrefaction; for he supposes that spreading sickness does not proceed from all kinds of corruption, but from that alone which emits vapours having the qualities of arsenic, wolf's-bane, or aconite, which being totally hostile to the healthy and vital spirits, are the whole cause of the venom.

V. In the like manner Francis de la Boe Sylvius, although he has most diligently treated on the stagnant and corrupted waters which excited a raging epidemic in Leyden, is nevertheless wholly silent on the verminose quality of the exhalations. He contents himself with discoursing at large on the vapours, which, on being exhaled from the waters, remain in the lower part of the atmosphere, and never ascend to the region of the clouds. These vapours, he thinks, are of a salso-sulphureous nature, and shews, that when mingled with the ambient air within our bodies, they are highly noxious.

VI. My much respected friend, D. Rammazzini, very laudably followed the path trodden by that excellent writer. In his tract on the epidemic distemper of Modena, he relates how it arose from a rainy season and stationary waters. He entertains no doubt that the atmosphere was defective in volatile parts, and tainted with many acid and earthy exhalations rising from a muddy, and, as it were, a fermented soil. These being inhaled, and applied to the vital parts, brought on the febrile diathesis. This diathesis, he conceives, is produced by a departure of the blood from its natural sweetness and fluidity, and by its becoming more thick and concrete, by which the balance is destroyed between the bile and pancreatic juice, the former being rendered weaker, and the latter stronger.

VII. Frederic Hoffman espouses the sentiments of Rammazzini. That man of admirable learning, when treating on the mischief of moist seasons, says, *the cause of all those accidents is justly and properly traced to the atmosphere rendered sluggish, heavy, and destitute of elasticity by marsh*

effluvia. Hence it is incapable of enlivening and intimately expanding the blood and juices, but favours the relaxation of the fibres, whereby they become flaccid. By this means the circulation and distribution of the blood are weakened. And when the circulation is retarded, the secretions and excretions are also diminished, whence the blood grows thick, viscid and gross. In consequence of this, many foul humours are accumulated in the body, and beget a predisposition to the before-mentioned diseases, and particularly to putrefaction. And if the excretions should be slow, the blood flows in a feeble and languid current through the vessels, the excrementitious matters remain in the body, and by a mutual action upon each other they produce an intestine putrefactive motion, dissolving the whole texture of the humours: whence it happens, that all severe epidemic distempers, whether they are denominated malignant, contagious, or pestilential, and even the plague itself, arise from a putrefaction of the humours.

VIII. Hitherto we have been treating of almost every thing that has been published on the effluvia of stagnant waters. There are however some considerations to be offered, which strike us with the air of much higher probability.

CHAPTER ELEVENTH.

Some preliminary considerations to our opinion of marshy effluvia being partly inorganic, and partly organic and animated.

I. That we may not incur the censure of an obscure, or too prolix a writer, we wish to submit some animadversions to our readers, before we venture to state what our doctrine explicitly is.

II. In the first place, the exhalations from marshes are not exactly alike at all seasons of the year, nor in all countries, nor in all collections of water; nor do they bring on the same diseases; but they are of very different qualities, and evidently excite various diseases, according to the various differences in the premises, and their degrees of mixture; for the vicinity of the foulest marsh is mostly harmless at mid-winter, because, at that time, nothing noxious exhales from the surface of the waters and of its neighbouring soil, but all the particles that would be injurious to man are repressed and kept down by the rigid contact and weight of the atmosphere.

III. What though the warmth that is produced by the solar rays in certain recesses, even during the prevalence of cold, is remarkably favourable to valetudinarians, and cures affections of the lungs! On the other hand, those very exhalations are the more noxious, malignant, and pestiferous, in proportion as summer verges toward autumn: especially if the waters lie toward the south, and are replete with aerial and terrestrial bodies undergoing putrefaction. It is therefore stated by writers, that these epidemics were seldom exactly alike, either as to the kind and seriousness of the symptoms, or their termination in recovery or death. It must therefore be acknowledged, that the vapours proceeding from stagnant waters are commonly noxious, and for the most part malignant, but that they cannot all be comprehended in one and the same species; and those physicians wander very wide of the truth, who seek, with the expectation of finding, one certain nature of particles in all effluvia, or who flatter themselves they have made such a discovery.

IV. Secondly—That, in a very doubtful case, we may have something fixed and certain, attention must be paid to the order and progress of the sickness which usually afflicts the inhabitants of marshy regions, and new comers, from the beginning of July to the winter solstice. For during those months we shall find the various forms of diseases defined by their variety of symptoms; especially if the marshes incline to the south (as is the case with many in Italy) and are overspread with a manifold and dangerous mass of minerals, decaying seeds, leaves, and especially of carcases and insects; wherefore, in the beginning of summer, the fevers are commonly certain and not malignant; with the increase of the heat they become continued, and are more fatal, and grow much worse, and even pestilential, about the autumnal equinox, especially if rains, clouds, mists, and southerly winds have prevailed: then, near the winter solstice, they universally abate their violence, but turn to chronic affections; for they who are liberated from these camp-fevers are almost always tormented with obstinate obstructions of the bowels, and with long-continued quartans.

V. Thirdly—We observe in muddy and marshy places, that fogs and jack-o'-lanthorns, fetid vapours, and other exhalations of various kinds, are plain to the naked eye. These are not organized and living bodies, but particles of the same waters, mixed and confounded with minute particles

of other things, that have been long soaking in its sluggish collections. From such a watery manufactory, fortuitous combinations and associations of congruous elements are formed, which are raised at length into the atmosphere by the sun's influence, in the same manner that mephitic vapours are elevated from their sulphureo-arsenical veins by subterranean fires.

VI. Lastly—We must not omit here to state, although it has been mentioned elsewhere, that marshy water, during the hot season of the year, is a most productive source of insects. Swarms of them take wing and infest the air, acting as forerunners of popular distempers, as Valeriola, Plenissius, and Hoffman have remarked. The same has been observed by myself, not only in one, but in many places. Besides, malignant fevers engendered by the blasts of marshes, are always accompanied with a great number of the worms called *lumbrici*; nor was there ever an extensive epidemic during which there was not an abundance of those worms or *vermin*, as Hippocrates calls them. Of these worm-fevers we have the testimony of Dominick Ravicinus, formerly physician to the Duke of Guastalla, in an Italian book which he wrote, on the morbid constitution caused by the excessive overflowings of the river Po. The same is confirmed by Bernard Rammazzini, in his elegant description of an epidemic fever from a filthy soil; and I myself can testify, that, in the year 1695, an epidemic prevailed at Rome, induced by sluggish and corrupted waters: of this, the history will be given in the first book of our epidemics.

VII. With these observations, I have thought proper to pave the way to a more diligent investigation of the nature of marshy exhalations. These are divisible into two kinds; to wit, those that are inorganic and inanimate, and those that are organized and alive.

(To be continued.)

CASE OF SUBCLAVIAN ANEURISM, *which occurred in Guy's Hospital, London; communicated to Dr. MILLER, by VALENTINE MOTT, M. D. Corresponding Member of the Medical Society of London, &c.*

NEW-YORK, Third Mo. 22d, 1810.

DEAR DOCTOR,

I SEND thee an account of a case of *Subclavian Aneurism*, in which I assisted ASTLEY COOPER in operating; which, I believe, was the first time that the operation of tying the artery above the clavicle has been attempted. If, after perusing it, thou shouldst think it worthy of a place in that scientific journal, the *Medical Repository*, it is at thy service.

Accept, dear Doctor, assurances of my high esteem and respect.

VALENTINE MOTT.

ON the 20th of 8 mo. (August) 1809, a man, aged 40, came into Guy's Hospital, in London, with a tumour, occupying the whole of the left shoulder, the greatest part of the clavicle, and extending under the pectoralis major muscle. It was not red upon the surface, but very hard, and without any distinct pulsatory motion: it was of about six months duration, and, when very small, A. Cooper said he saw it, and there was no distinct pulsatory motion to be discovered; at least, only such a motion as the subclavian artery beneath might communicate to a tumor situated immediately over it. The tumor, however, A. C. fully believed to be an aneurism of the subclavian artery; and when, upon examination, an aneurism was discovered in the femoral artery, just below Poupart's ligament, the smallest doubt did not remain, in the mind of any person present, as to the nature of the tumor in the shoulder, and that it was an aneurism of the subclavian artery.

The situation of the man being truly painful, and it being evident that the disease must prove, in a short time, fatal, if no operation were to be performed, A. C. was determined to make an attempt to take up the subclavian artery, just after it had passed betwixt the first and second scalenus muscle.

Though this would appear to many to be a cruel and unwarrantable attempt to save life, yet, as A. C. very properly observed to me, it could only shorten his days a little to attempt the operation, and it was possible it might succeed, though it had never before been performed.

The man was willing to submit to any thing that might be thought proper for the relief of his distresses. A. C. then pointed out to him the uncertainty of the operation, and promised, if he would submit to it, that nothing should be done but what was perfectly proper and safe; saying, that, if in the course of the operation he should find it not safe to proceed, he would give it up. The man consented, and was laid upon the table in the theatre, with his shoulders a little elevated. The operation was then begun, in the presence of G. W. Young, Esq. Surgeon, B. Travers, Demonstrator of Anatomy, and a number of other Surgeons. The incision was commenced at the outer and lower edge of the sterno-cleido-mastoideus muscle, close to the clavicle, and carried, straight outwards and backwards, about three inches. The most careful dissection was now necessary, and by means of the edge and sometimes the handle of the scalpel, the muscles were separated, till the nerves, going to form the axillary plexus, were laid bare. The opening between the muscles was very small, and so deep (*A. C. remarked that it was like looking down a well*) that the fore finger could but just reach the nerves. The subclavian artery was felt beating very feebly, immediately under one of the large nerves going to the axilla; it could not be felt at all by several that were present, and by none constantly; A. C. was convinced that he felt it at times, and I was certain that I perceived it also. A curved probe was now passed under the artery, and repeated trials were made to draw it from under the nerve, so as to pass a ligature around it; but these were all unsuccessful. Every time the nerve was put upon the stretch, with this view, the patient complained of the most excruciating torture, not only in the shoulder and neck, but extending throughout the whole arm. It was not one or two trials, but many, that were made, before A. C. could be satisfied to relinquish the operation. After, however, keeping the man on the table an hour and fifty minutes, he desisted from any further attempts; saying it was impossible to accomplish it, and even if it were then possible, after so much violence had been done, and the patient so much exhausted, it would not be safe, as it was most probable that it would almost immediately prove fatal. A. C. remarked to me, that the operation, though not difficult in a small aneurism, cannot be performed in one of a very large size.—The man did not lose an ounce of blood in the attempt.

The wound was now brought together by sutures and

plaisters; the patient put to bed, and a large opiate given him. He complained of extreme pain all over his shoulder and arm, occasioned, no doubt, by the violence done to the large nerves, going to form the axillary plexus. A considerable degree of fever ensued the day after the operation, which very much increased; attended with high delirium, though venesection, purging, and sudorifics were assiduously used; and on the sixth day from the operation he died.

Upon examining the body after death, the two first ribs were found to be destroyed, and a portion of the upper lobe of the left lung was adhering to the aneurismal sac; the sac was large, and contained large coagula of blood, which had thrust the clavicle very much upwards. A. C. took out the part, very carefully preserving all the vessels connected with it.

Would any but a great mind, conscious of its own powers, and the rectitude of its intentions, make the following remarks? He said to me, "I am suspicious that, in this operation, the thoracic duct must have been divided, as it was on the left side; though I did not think of it at the time of the operation, nor before it." I could not learn that any person present had thought any thing about the danger of injuring this vessel; no doubt from its being a vessel which we have never been accustomed to think of in any operation. I regret that it is not in my power, at present, to satisfy the curious on this point, as A. C. had not ascertained the fact when I left London.

We are not to despair, though this first attempt* has been unsuccessful, when we consider the great and splendid chirurgical achievements of the last three years in the British metropolis. The first operation for carotid aneurism was performed by that eminent and accomplished surgeon, A. Cooper, and was unsuccessful: this, however, did not deter him from a second attempt, in the summer of 1808, which completely succeeded.

After this, the carotid was taken up by an eminent surgeon of Stockholm, M. Bierken; but, from some unfavorable circumstances of the case, it failed, as I am informed in a letter from my learned friend Dr. Wegell, Physician to the late King of Sweden, who assisted at the operation, and

* KEATE, the Surgeon-General of the British army, and one of the Surgeons of St. George's Hospital, is said to have taken up this artery, below the clavicle, in a wounded soldier, and he recovered.

accompanied me, when in London, to A. C's second operation.

In the winter of 1809, HENRY CLINE, senr. of St. Thomas' Hospital, took up the carotid for an aneurism of a very large size, involving the posterior angle of the lower jaw, and extending down towards the shoulder. The artery was secured in the usual way, by that great surgeon, and without any kind of difficulty. The man, in the course of the following night, drank very freely of spirits, and became, in some measure, inebriated, and he died the next day. The parts were examined after death, and there was nothing unusual about the aneurism, but the brain and its membranes shewed signs of inflammation. This state of the brain, no doubt, was induced by the excessive stimulation, and caused his death; as the brain then, from its altered circulation, was more predisposed to inflammation.

The carotid was again tied in the spring of 1809, by B. TRAVERS, Demonstrator of Anatomy at Guy's Hospital, for an aneurism by anastomosis, situated in the left orbit, which had protruded the eye a little from its socket. He used two small round ligatures, but did not divide the artery between, and secure the ligatures by passing them through the artery, as was done in the other cases, except A. C's first. The ligatures came away in about twenty days, and no hæmorrhage ensued, nor did the brain suffer the least injury. The pulsation in the tumor was diminished by the operation; there was, however, but little alteration in its size, three months after. Though this operation did not succeed in removing the disease for which it was performed, it is a valuable fact, and proves, with A. C's case, that the artery may be tied with perfect safety as to the functions of the brain.

Abernethy's operations upon the external iliac, and A. Cooper's upon the carotid and subclavian, must be admitted by every one to be master-strokes of scientific surgery. These, most undoubtedly, are proud days for London, and particularly, when we know that they have never been the subjects even of dream or speculation, in the capital of France.

Are we not to expect, from these and similar examples, that the lives of many valuable individuals may be protracted far beyond the span which has hitherto, without exception, proved fatal? It may, indeed, be protracted to a very late age, if we are allowed to judge from similar cases, in which the whole system has not become affected from the disease of a part.

ABSTRACTS *from the* BILLS OF MORTALITY *kept in the City of New-York, during 1807 and 1808; by* SAMUEL L. MITCHILL; *being a continuation of the Summary made by him on a former occasion.*

IN a late volume, (M. R. Hex. 2. Vol. v. page 32,) I published an abstract of the New-York Bills of Mortality for 1804—5 & 6. They have been so favourably noticed, that I have availed myself of the care and industry of Mr. PINTARD, the City-Inspector, to make a similar digest of the returns for 1807 & 8. That gentleman observes, that the reports of deaths in the city and county of New-York are perfectly accurate; but that physicians ought to be more attentive in giving their certificates of the diseases of which their patients died. If the medical attendants would be more particular on this point, we should be enabled to form a more correct judgment of the diseases which destroy life, than at present; and, under a continuance of the existing regulations, a great body of the most valuable information will be collected.

Consumption still vexes our society with its unrelenting severity. During the last two years, somewhat more than *one fifth* of those who died, including infants, were cut off by that cruel distemper. When other pulmonic diseases are taken into the account, they amount to rather more than a *fourth*. And when to these are added the maladies intitled “debility” and “decay,” they make *almost a third* of all those mortal enemies of our species.

The *Croup* continues to carry off a considerable number of persons, chiefly children. The seat of this disease in the windpipe and bronchia, would almost lead to a natural arrangement of it with the catarrhal affections, which so generally precede and accompany consumptions. How far the disorder may be connected with a maritime atmosphere, or with the habit of lulling infants with paregoric draughts, are matters that deserve serious consideration.

There has not been a single capital execution within the last two years. This at once evinces the lenient spirit of our laws, and the possibility of supporting the order of society without the employment of sanguinary punishments.

In order to estimate the rarity of homicide, it will be recollected that three acts of this kind have been committed

during two years. There is no military force to patrol the streets, or to sustain the vigor of the laws: there is no other guard than a night-watch, composed of citizens, hired to walk the streets between bed-time and break of day, and apprehend disorderly persons. But these watchmen are not soldiers; nor do they carry military weapons. The Americans are not prone to satiate their revenge by murder.

Failures and mistakes about the prevention of small-pox by vaccination, have, with some persons, operated in favour of the variolous practice. The difficulty that has now and then occurred, of keeping a steady supply of the cow-pox matter; the deceptions that sometimes occur in taking and preserving it; and the disappointment and vexation experienced in consequence of small-pox in persons who had been supposed to have undergone vaccination, have induced a persuasion, in the minds of some people, that it is better to inoculate for the small-pox at once, and put an end to their apprehensions. But a great majority of the most respectable practitioners in this city, still continue to feel an undiminished confidence in the anti-variolous powers of the cow-pock; and the most authentic accounts of European experience seem to confirm this opinion. The philanthropists who labour so zealously to preserve human life, ought to exert themselves with equal energy to increase human subsistence: for unless the means of living shall be provided for those who are saved from the ravages of small-pox, they are but reserved, after the utmost sufferings of want and misery, to become, at last, the victims of some other disease.

Suicide continues about as frequent as heretofore. The law provides nothing concerning the body, estate, or relations of those who take away their own lives; and experience proves, that the restraints of morality and religion are not sufficient, in all cases, to reconcile individuals to a natural death.

Of children, almost one fourth of all that are born, die before the first year; and almost *one third* perish before the completion of the second.

TABLE I.

Yearly deaths in New-York.

Years.	Men.	Women.	Boys.	Girls.	Total.
1807	741	529	520	522	2312
1808	618	437	506	453	2014

TABLE II.

Monthly Deaths in New-York.

Year.	1807	1808
January.	158	162
February.	166	172
March.	180	224
April.	215	177
May.	145	137
June.	127	131
July.	195	179
August.	328	184
September.	286	180
October.	172	133
November.	158	151
December.	182	184
Total.	2312	2014

TABLE III.

Relative Ages of those who die in New-York.

Year.	
1807	537
1808	435
Those who die under 1 year.	
1807	208
1808	224
Between 1 and 2 years.	
1807	176
1808	161
2 and 5.	
1807	78
1808	93
5 and 10.	
1807	111
1808	119
10 and 20.	
1807	295
1808	244
20 and 30.	
1807	319
1808	271
30 and 40.	
1807	218
1808	187
40 and 50.	
1807	165
1808	114
50 and 60.	
1807	97
1808	75
60 and 70.	
1807	68
1808	64
70 and 80.	
1807	34
1808	24
80 and 90.	
1807	5
1808	3
90 and 100.	
1807	1
1808	0
100 and upwards.	
Total.	
1807	2312
1808	2014

Names of the Accidents and Diseases that induce or accompany Death in New-York.

TABLE IV.

Year.		
1807	467	Consumption.
1808	429	
	59	Debility.
	43	
	112	Decay.
	89	
	16	Influenza.
	2	
	46	Pleurisy.
	44	
	58	Inflammation of lungs.
	40	
	4	Spitting Blood, or Hæmoptylis.
	0	
	10	Cold.
	14	
	4	Rupture of blood vessels.
	6	
	3	Asthma.
	5	
	76	Still-born, or Abortion.
	64	
	162	Infantile Flux, or Diarrhœa.
	120	
	101	Croup, or Hives.
	94	
	34	Aphæ, or Sprue.
	16	
	35	Eething, or Den-tition.
	37	
	35	Whooping-Cough, or Pertussis.
	35	
	60	Worms.
	43	

Affections chiefly Pulmonic.

Diseases chiefly

TABLE IV. CONTINUED.

Year.		
1807	219	Convulsions or Fits.
1808	159	
	30	Dropfy of the Head, or Hydrocephalus.
	14	Intemperance.
	1	Murdered.
	1	Infanticide.
	16	Suicide.
	1	Manslaughter.
	9	Colic.
	8	Cholera Morbus.
	4	Cramp in the Stomach.
	7	Diarrhœa.
	30	Dysentery.
	29	Inflammation of bowels.
	14	Inflammation of stomach.
	18	Diseased liver.
	1	Dispepsia or Indi- gestion.
	6	Burns and Scalds.
	31	Drowned.
	34	Sudden death.
	21	

*Infantile.**Moral Infirmities.**Intestinal Disorders.**Casualties.*

Year.		
1807	1	Drinking cold water.
1808	1	
	2	Frostbitten.
	0	
	4	Fracture.
	2	
	27	Other Casualties.
	24	
	9	Bilious.
	3	
	3	Inflammatory.
	1	
	3	Intermittent.
	5	
	3	Hectic.
	3	
	2	Putrid.
	3	
	3	Remittent.
	10	
	106	Scarlet.
	62	
	162	Typhus.
	120	
	29	Smallpox.
	62	
	1	Measles.
	64	
	10	Inflammation of brain.
	17	
	1	Inflammation of bladder.
	0	
	4	St. Anthony's fire, or Erysipelas.
	0	
	17	Sore-throat, or Cyn.
	13	
	5	Quinfy, or Cynanche.
	4	

Casualties.

Febrile Disorders.

TABLE IV. CONTINUED.

Year.		
1807	7	Abscess.
1808	6	
1807	1	Aneurism.
1808	0	
1807	38	Apoplexy.
1808	23	
1807	5	Cancer.
1808	4	
1807	0	Carbuncle.
1808	1	
1807	0	Caries.
1808	1	
1807	11	Childbed.
1808	20	
1807	1	Fever puerp'l.
1808	3	
1807	0	Leucorrhœa.
1808	1	
1807	3	Menorrhœa.
1808	0	
1807	19	Chorea.
1808	14	
1807	1	Diabetes.
1808	0	
1807	98	Dropsy.
1808	91	
1807	8	Epilepsy.
1808	0	
1807	1	Gout.
1808	0	
1807	0	Hernia.
1808	3	
1807	9	Icterus or Jaundice.
1808	13	
1807	8	Insanity.
1808	7	
1807	2	Locked Jaw or Tetanus.
1808	4	
1807	1	Leprosy.
1808	0	

Sexual.

TABLE IV. CONTINUED.

Year.	
1807	17
1808	20
Mortification.	
2	0
Nervous Head-ach.	
51	49
Old age.	
23	16
Palsy.	
4	5
Rheumatism.	
0	1
Rickets.	
4	4
Scrophula.	
0	1
Scurvy.	
2	2
Spasms.	
2	0
Stone or Calculus.	
15	10
Syphylis or Pox.	
4	1
Ulcer.	

TABLE IV. CONTINUED.

An ACCOUNT of the YELLOW FEVER, as it appeared at STABROEK, in the COLONY of DEMARARY, during the principal part of the years 1803 and 1804. By Dr. WILLIAM FROST, physician at that place; addressed to Dr. JEREMIAH BARKER of Portland, and by him communicated to Dr. MITCHILL.

(Continued from Page 257.)

THE apartments of the sick were always kept as airy as possible, without the admission of a stream of air directly over the beds whereon they laid.

The patient's drink consisted of teas made of balm, or Virginia snakeroot, chamomile flowers, &c.; a beverage of acidulated tartar of potash, tamarind water, toast and water, porter and water, sengrie, orange and lemonades, cyder and water, and sometimes cold water. After a patient had taken a draught of any thing in order to quench febrile thirst, at a time when the stomach was irritable, I always directed him to lie on, or incline himself, to the right side; because the contents of the stomach, pressing against the superior or upper orifice of that organ, when it was in such a high state of excitability, always impelled immediate efforts to vomit, which will inevitably take place if the patient lay on the left side, after taking a considerable draught of liquor of any kind. I experienced the pernicious and inconvenient effects of lying in this posture, in an attack of fever that I was seized with soon after my arrival at Stabroek; I found it to occasion nausea, and reachings to vomit, the very instant I attempted to lie on the left side; but as soon as I altered my position to the right, the nausea, &c. subsided.

Their *diet*, or nourishment, consisted of sago boiled in milk, and sometimes wine was added to the sago; boiled rice, Indian gruel, when meal could be obtained; beef, lamb, veal, or chicken soups, tapioca, salep, and the like.

The *cinchona* was always given in Madeira wine the instant the fever remitted, or the moment the patient was able to bear it, in any form, after the remission. But this seldom happened until the violence of the symptoms had much abated. It was always preferred, in substance, to any other form, if the stomach would retain it exhibited in that manner; if not, in decoction or tincture, joined with acid vitr.

arom. or laudanum, as occasion required; I generally joined cortex. aurant, and rad. serpt. virg. to the cinchona, when given in decoction.

The infusion or decoction of columbo, joined with cort. quassiæ, was found an admirable tonic, and in many instances was more congenial to the stomach than the cinchona, given in any form. The cortx. angusturæ superceded the use of the cinchona in a variety of cases, where the latter remedy could not be retained; but the efficacy of the *cold shower bath*, in those low, morbid, irritable, gastric cases, on the decline of the fever, cannot be too much eulogized as a tonic.

It is probable, Sir, you will expect me to make some observations on emetics. The following remarks are the result of my experience of emetics, as a remedy in the yellow fever within the tropics.

During the two years that I was in the navy of the United States, viz. 1799 and 1800, and cruizing principally on the coast of Surinam and Cayenne, several cases of yellow fever came under my care, not only on board the Portsmouth, (the ship to which I was attached), but also on board several American merchant vessels. By the particular request of some of my patients, they being confident of "*a redundancy of bile* on the stomach," as they said; emetics were administered in the incipient stage of the fevers, and not one in six persons recovered who took them, although phlebotomy was sometimes premised. The Lieutenant of Marines was the first person who was attacked with this fever on board the Portsmouth; and which was occasioned by an imprudent exposure, late at night, to the air and heavy dews of Paramaribo, in Surinam. He took an emetic of 3 grs. of tart. antim. by his own particular request, within eight hours after his attack, and he continued to vomit almost incessantly, until he died, which was on the third day of the disease. He vomited matter like coffee-grounds twelve hours before death. The whole round of anti-emetics, applicable to his case, were resorted to; but all were insufficient to rescue a valuable man from the shafts of death. He and one more were, however, all the persons who died of this disorder on board the Portsmouth, during that period of time.

Several other patients, who fell under my observation,

insisted on taking emetics or nothing; although the most cogent reasons were adduced to discourage them from their use, and several cases were mentioned to them of the uncertain effects of emetics in tropical regions. These cases nearly all terminated as just recited. Profuse and fatal vomiting sometimes, however, took place, where no emetics were given. These fatal cases happening after the exhibition of emetics, and all nearly at the same time, determined me not to give any more; having, as I thought, seen sufficient proof of their unsalutary effects, on Northern men, in low latitudes. I then adopted a permanent mode of practice, which was the depleting system, in the manner already related in the foregoing account; but the subsequent treatment was by mercurials, as recommended by Dr. RUSH. This plan of practice was tolerably successful; but many died before the mercury could be made to affect the glands of the mouth, the fever advanced with such rapid strides.

On my passage out to Demarara, we fell to leeward of our port, and were necessitated to go to the Island of Trinidad. It was three or four weeks before I could get a passage to Demarary, and during my detention there I was called on to visit several Americans, sick of this fever. I thought this a good opportunity to try the effects of alkaline emetics, in the manner you used them in Portland in 1801—*Med. Rep.* v. 6. In three cases where emetics were given in that form, and in the incipient stage of the disease, their effects in inverting the stomach and intestinal canal, were similar to what I had experienced on the coast of Surinam three years before, and two of the patients out of three died; although I persevered to the last hour of their existence in the use of those means recommended subsequent to the use of alkaline emetics. The third patient recovered, but with the utmost difficulty. I imputed his recovery to the use of the cold bath and anodyne enemas.

Soon after my arrival at Demarary, in conversation with some medical gentlemen lately arrived from Holland, who attended the Dutch troops stationed at Stabroek, I expressed a wish for them to try the effects of alkaline emetics, to see how they would succeed in other persons' hands, besides my own, within the tropics. As the yellow fever was then very prevalent among their troops, and considerably mortal, a favourable opportunity presented to suggest to them some new mode of practice, which might possibly be more suc-

cessful ; and as alkalies had never been resorted to by these gentlemen as remedies in this fever, I intimated to them at the same time, the happy effects that had followed the exhibition of alkalies in the fevers peculiar to the cities in North America, and related to them the outlines of the theory of *febrile diseases*, by Dr. MITCHILL of New-York, and other persons in the United States ; the ingenuity and reasonableness of which struck them very pleasingly. They told me afterwards that they tried the alkaline emetics, and I found with the same effects as occurred to me in Trinidad. I then suggested the idea of premising mercurial or other brisk cathartics, omitting the emetics altogether, and then to pursue the alkaline mode of treatment, to which they consented ; and I was afterwards informed by them, that this method of treatment was attended with infinitely better success than any mode of cure they had ever attempted before.

On the whole, I am confident that emetics ought not to be administered in the fevers of low latitudes, even of the mildest kind, unless given before the disease has assumed a specific form, or before "the apparent inflammatory diathesis" has taken place, and *then* ought to be composed of *Ipecacuanha* and some cathartic medicine joined with it, with a view to counteract the extreme inversion of the stomach and alimentary canal, so peculiar in this terrible fever, in the latitudes just mentioned. Emetics, even of a mild nature, have too often been attended with the consequences that I have already enumerated ; and I feel no hesitancy in saying, that, notwithstanding their salutary effects in the Eastern, Middle, and Southern States, they have been the cause of many a sailor's death, in consequence of their indiscriminate and injudicious use, by commanders of vessels, in tropical climates. In support of their ambiguous effects in tropical countries, we have Hume, Hillary, Hunter, Mosely, Holli-day, &c.

Dr. JACKSON prescribed emetics, however, in the common *endemic* or *remittent fever* of the island of Jamaica, (which is peculiar to those persons who are inured to the tropics, the stranger, from high northern latitudes, seldom being favoured, in his first attack of fever, with the remittent endemic form, and it is well known the stomach does not possess that accumulated degree of excitability, in a person inured to the tropics, that it does in one unassimilated to that climate) but they were not exhibited without the precaution of joining cathartic salts with them at the same time,

and "immediately after the operation of vomiting was finished, a blister was applied to the region of the liver." The Doctor observes, relative to *emetic tartar*, even in the endemic fever, that "it was often found to be dangerous, scarcely ever effectual in cutting short the course of the disease, unless given at an early period, or before the fever had assumed a proper form." And again, on the same subject, "instructed," says he, "by repeated examples of their hurtful effects, (emetics), I at last scarcely ever employed antimonial vomits; even the safest kind were used with caution. If it appeared at any time that the action of vomiting would be serviceable, chamomile tea, or, at farthest, a few grains of ipecacuanha were generally thought sufficient for the purpose."

And Dr. CLEGHORN, after pointing out the "dangerous consequences of emetics," given in the *tertian fevers* peculiar to the island of Minorca, observes, "*if vomits are to be used*, they should be given in the *beginning* of the disease, before repeated paroxysms have brought on inflammations, &c. in the bowels, which too frequently accompany tertians, and that emetics exasperate this inflammatory diathesis beyond expression."

(To be continued.)

CASE of ANEURISM, cured by spontaneous concretion of the sides of the artery. Communicated to Dr. MITCHILL, by LYMAN SPALDING, M. D. of Portsmouth, (N. H.)

PORTSMOUTH, September 12, 1809.

To the Hon. SAMUEL L. MITCHILL.

SIR,

I HAVE inclosed an account of the *spontaneous cure of Aneurism*. It is proper for me to state that it was read before the New-Hampshire Medical Society; and in consequence of the Messrs. Bell's stating that a spontaneous cure of Aneurism was never known, I transmitted to them this account; but as I do not know what disposition they have or will make of it, you have my liberty to make such use of it as you please.

LYMAN SPALDING.

APRIL, 1804.—Deacon ROBERT WELLS, aged 65 years, of Wells, in the District of Maine, perceived a swelling

about the middle of the left thigh, on the course of the femoral artery, nearly the size of a hen's egg.

In November, 1806, he first noticed a pulsation in the tumor, which was much increased by violent exercise; and in January, 1807, he perceived a coldness and numbness adown the inside of the knee, to the ankle and foot.

September, 1807—The tumor becoming troublesome, Mr. Wells, of his own accord, applied a bandage for a few days, which rendered it more uneasy, and caused it to swell, as it had often done before, for three or four days, and then subside, always gaining a little in every intumescence.

On the 30th of November, 1807, he thought it expedient to have medical advice. His family physician, Joseph Gilman, of Wells, was called. The tumour had now increased to the size of a man's fist; the pulsation was very strong, perceptible both to the eye and touch.

The first of January, 1808, the pulsation in the tumor was so strong, as to be perceptible to the eye through his clothes. He again applied a straight bandage. The first day it seemed to support the tumor, producing an agreeable sensation; but on the second, the tumor swelled to twice its former size, and afterwards became excessively painful, obliging him to remove the bandage that night. On the removal of the bandage, the pulsation, which was so prominent a symptom the day before, was now no more, and was never afterwards perceptible to the eye or touch. The coldness and numbness, on the inside of the leg, were now much increased, and continued till the operation, when they gradually subsided.

The tumor continued to increase in size for a few days, after the removal of the bandage, and never afterwards subsided so fully as it had done before. After this paroxysm he began to grow better; and by the middle of January he could move the leg with much more ease than formerly, and perform more labour than he had done for months.

On the 25th of March, the tumor became painful, and afterwards inflamed. The pain and inflammation continued to increase in violence, (becoming extremely excruciating,) till the 28th, when the pain subsided; and on the 29th, the top of the tumor, for a space as large as a man's hand, was covered with vesications. These, in a few days, discharged their watery contents, and exposed the true skin, black and mortified, about the size of a dollar, which continued to increase in size till the operation.

On the 6th of April, 1808, in company with Doctors William Cutter, James H. Pierrepont, Joseph Gilman, and others, I visited Mr. Wells. The tumor was as large as a child's head, its top sphacelated, and sides discolored, by the spreading of the disease. Some of the gentlemen thought they discovered an undulatory motion in the tumor; to my fingers it was not perceptible; but from the once evident pulsation, site, colour, and consistence of the tumor, we were satisfied of its nature.

An operation was agreed on. The tumor was found to extend so high into the groin as to leave but just room to apply the tourniquet.

Every preparation having been made to meet a profuse jet of blood, Dr. Cutter commenced the operation by dividing the integuments, and laying open the whole length of the tumor: about two quarts of black grumous blood rolled out, but, to our astonishment, not a particle of florid arterial blood appeared. We dove down to the bottom of the cyst, turned out all the clots, sponged it, and with eagle eyes awaited the slacking up of the tourniquet. It was completely loosened by slow and gradual turns; but no jet of arterial blood appeared, no pulsation, or oozing of blood, was perceptible at the bottom of the wound; but astonishment was depicted on the countenance of every one.

It appeared to us impossible (the patient not being faint, and tourniquet loose,) that an aneurism of the femoral artery could remain open without an immense loss of blood.

Mr. Wells was placed in bed, with a light napkin only over the wound, while we gave scope to physiological speculations. Two opinions were advanced; the one that a natural adhesive process had taken place in the coats of the artery, both above and below the sac: the other, that a clot of blood had filled up the calibre of the artery, which would be expelled when the circulation had acquired its accustomed activity.

About an hour and an half after the operation, the patient was removed to his seat again. The tourniquet having been perfectly loose all this time, opportunity had been given for the circulation to acquire its usual vigour. The wound was sponged anew, but not a drop of arterial blood was observed. We examined the inside of the cyst with more care and attention, and soon discovered, that the lining of the cyst had the same appearances, and was nothing more nor less than the internal coat of the femoral artery; and the

artery itself was discovered leading into and out of the tumor, into which a probe could be passed several inches only.

This was proof positive that the calibre of the artery had been obliterated, both above and below the tumor, by a natural adhesive process. A similar instance was not recollected on record. Charles Bell, in his *Operative Surgery*, vol. I. says; there is not on record an instance of the spontaneous cure of an aneurism.

The bottom of the sac was in the natural bed of the artery; this, and two thirds the way up the sides, appeared to be a dilatation of all the coats of the femoral artery. The superior third had a much thinner cyst, and from its diseased state, the sphacelus having extended to it, we were not able to determine whether this was a continuation of the coats of the artery, or that they had been ruptured, and this part of the cyst formed from the common integuments.

The wound was dressed without a single ligature on the artery, or a division of it, and the patient placed in bed.

When the tumor was opened, there appeared a singular substance, about the size and shape of a man's open hand, placed longitudinally, one edge being towards the bed of the tumor, and the other towards its surface. This was in the midst of the grumous blood, and not in contact, or connected with any part of the cyst. It was of a light florid colour, with longitudinal fibres, of the consistence of long boiled beef, and, with the same ease as that substance, might be shred into fibres. It is probable the gluten of the blood had been converted into this fleshy mass, by having been forcibly driven through the aneurism. This process, Ruysch often demonstrated by the whipping of blood.

The thirteenth day after the operation, that part of the integuments which had been discoloured, together with some of the adjacent parts, sloughed off, leaving the lips of the wound five inches asunder, which were brought together in some measure by adhesive plasters.

The eighteenth day, Dr. Gilman, the attending Surgeon, was called in great haste, a profuse hæmorrhage having taken place from the wound. Three pints of blood were supposed to have been lost; but the hæmorrhage had ceased before the surgeon arrived. On removing the dressings, it was perceived that the blood had issued from a vessel that

had been ruptured by the separation of the mortified from the sound parts.

Two days afterwards there was another slight hæmorrhage: the debility induced by these was very great. The wound now healed kindly; and by the eightieth day was completely cicatrised, and his health and vigour in some measure re-established. Six months after the operation, Mr. Wells thought he enjoyed better health than usual, and was able to ride half a dozen miles on horseback.

On reviewing this case two very singular circumstances present themselves: the fleshy mass in the coagulated blood; and the obliteration of the calibre of the artery, whereby a spontaneous cure of the aneurism had taken place.

That the calibre of the artery had been obliterated, does not admit of a doubt. The probe was repeatedly passed into it from the aneurismal sac, and we could plainly discover, both above and below the tumor, the cavity of the artery, terminating in a cone, beyond which the point of the probe could not be forced.

The obliteration of vessels, and the destruction of parts, are not unusual processes in the animal economy, when the original function of the organ has ceased. Thus, after birth, the ductus arteriosus and umbilical vessels are no longer necessary; therefore their cavities are obliterated, and they become mere ligaments.

In this case, the circulation through the aneurismal tumor was impeded, probably by the growth of the fleshy mass, and the blood was compelled to seek some other rout. The collateral arteries, by a principle in the animal economy, which has been well established by the writings of John Bell, were enlarged and anastomosed in direct proportion with the obstruction of the main artery; and no doubt the calibre of this artery gradually accommodated itself to the diminishing quantity of blood sent through it. The obstruction was therefore increasing, the collateral branches enlarging and anastomosing, while the circulation through the tumor was diminishing, and the bore of the artery contracting.

This process was gradually carried on, till the obstruction was complete; the anastomosing branches were so enlarged as to convey all the blood sent to the limb, and then the sides of the main artery adhered by a natural process,

a new circulation having been established, much in the same manner as after an operation for aneurism. This, at first sight, may appear to hold good as it respects the artery above the aneurism only, but if it be closed there, what should keep it open below?

The precise time when this process commenced or terminated, is not at all material for establishing any physiological fact relative to the case. An important change appears to have taken place the first of January, when the bandage was applied the second time. The tumor then increased very rapidly in size, the pulsation became extinct, and the coldness and numbness of the limb were much more considerable than at any time before. We should suppose that at this time there was a rupture of some of the coats of the artery, and that the circulation was formed, as the coldness and numbness, after this, began to abate, and since the operation have principally subsided.

Anatomical and Pathological Observations on the TEETHING of INFANTS; and on the analogy between INFANTILE DENTITION and some other Diseases: In a communication from HORACE H. HAYDEN, of Baltimore, Surgeon-Dentist, to DR. MITCHILL, dated August 25, 1809.

(Continued from page 225.)

WITH those who believe that the symptoms of teething are produced by the tension of the gums, and the investing membrane, (the latter of which, I shall contend, never rises above the edge of the alveolus or jaw,) the question may arise, how is the existence of this fluid, which is capable of producing these effects, to be proved? Every day's experience proves it—topical remedies, which are applied, tend to confirm it; for leeches and blisters serve only to divert the humours, and lessen the secretions into those cavities, and thereby lessen the irritation, for they have not the power of lessening a direct tension of the fleshy fibre of the gums. Various applications, recommended for the gums, serve, not to facilitate the cutting the tooth, but as an excitement, by which this *acrid* fluid is taken up, and the

diseased action removed. That of local beeding, if it answers any purpose, it is that of lessening the quantity of fluids, and the inflammation which is excited by the acrid fluids already secreted. The very appearance of the gums, is often such as to lead one to suspect the agency of some hidden cause; for there exists, during laborious dentition, not only high inflammation in the gums, but they are much inflated, puffed up, and a degree of fluctuation perceptible.

This fluid, by long continuation, assumes, no doubt, the form of pus, and constitutes, I presume, the principal cause of ulceration, imposthumation, &c.

In a late conversation with a medical gentleman, on this subject, he informed me that, in several instances, where he had been under the necessity of lancing the gums of children in laborious dentition, he had witnessed a considerable discharge of pus. We frequently see it in adults, about the *dentes sapientiæ*—but in this case it arises from two causes: the one is, in manner as before mentioned. When this is the case, on giving discharge to the matter, and the tooth appears through the gums, they become healthy, and the tooth firm, with no subsequent appearance of disease about the parts.

Pus is also produced, in some instances, about this class of teeth in the inferior jaw, by the tooth being closely confined under the ascending part of the coronoid process.—In this instance, the tooth, through want of space, is forced out in an oblique or inclined position; meeting with the second large molares, it is again forced backwards against the ascending process, so hard as to occasion a deperdition of the substance of the bone, with considerable uneasiness, and a discharge of pus, which the patient must submit to, or consent to the extraction of the tooth; for, in no *one* instance have I known this discharge stopped, even in cases of some years standing, until the tooth is removed. In order to prove more clearly the truth of this opinion, it is only necessary to witness the effects produced, in numerous instances, by this acrid fluid, on the teeth of children, who have been subject to that truly calamitous disease, the *measles*, during the second dentition, and while the teeth were in part above the jaw, and yet within the gums. In this instance, this acrid fluid, this morbid secretion, becomes capable of resolving the enamel almost entirely, and of corroding the tooth

all into holes,* and when the disease in the system subsides, and this agent is absorbed, or otherwise removed, nature makes another effort to reinstate or restore the tooth, and the remaining part of the crown appears, healthy, fair, and smooth.

These, Sir, are cases, that (in my opinion) afford striking proof of the reality of morbid, or acrid secretion, and its effects, whenever it happens in contact with *bone*.

Frequent are the opportunities I have had of witnessing the unpleasant effects of this complaint (the measles) on the teeth: therefore, I am inclined to think, it would be a fortunate circumstance, if some one could devise means to mitigate the evils, and lessen the deformities, too often attending it.

In order more fully to prove to you, Sir, the existence and reality of this agent, and its operations and effects, as well on our bones as on our infantile gums, I will beg leave to refer you to a case, which, while it amounts to a confirmation of the truth of my assertion respecting dentition, will afford you a curious specimen of the operation of *nature*.

In the head of a subject, about five years old, (or a little over or under, it is immaterial) if you can possibly procure one, you will see that at that age, the crown of a number of the second set of teeth is formed, the enamel of each has acquired a degree of consistence and hardness, such as, that no membrane or vessel can possibly attach itself to it: you

* Since the above was written, I have read Mr. Fox's treatise on the Teeth, in 2 vols. quarto, in the second of which, (plate 8, fig. 14,) he has given a correct and handsome specimen of the effects of this morbid secretion, during the prevalence of the measles; and I am not a little surprised to find, that the above author, who has enjoyed so great an opportunity of ascertaining the effects of diseases in the bones, (being Lecturer at Guy's Hospital,) seems to be ignorant of the cause of this deformity. He says, that "This defective formation of the enamel, is usually confined to the incisores and cuspidati, and *first permanent molaris*"—true; the first adult molaris makes its appearance through the gums, from the 6th to the 7th year, previous to which, or mostly so, the four incisores, in either jaw, are cut through; and it will be found that if the incisores are injured, the first molaris is also, and *vice versa*. If the person is subject to the disease, (that is, it is only cases of some degree of violence that produce this effect) after this age, the bicuspidates and second molaris, and perhaps the point of the cuspidati will be found injured, while the incisores are free from any mark of it. He says, that "it is very remarkable that this circumstance often occurs in several children of the same family"—vol. 2, p. 28. He should have observed, that it often occurs that several children of the same family have the measles at the same time. He says also, "No certain reason can be assigned, why the membrane secreting the enamel should so often deviate from its natural action," &c.

will see, also, that each tooth has its separate cell, or socket, which has, in life, its investing membrane. This little cell constitutes a cavity about the crown of each tooth, into which, it will be acknowledged, there is a constant secretion, as well as in all other cavities in the body.

That this secretion, receives a change, after being secreted, by being thrown in upon the enamel of the tooth, (which in this instance, I consider as extraneous, as it relates to the surrounding parts) I think most probable, though I do not state it as a fact; but that it possesses, or assumes a quality, capable of committing its ravages on the surrounding parts is certain;—whether necessarily, and for a wise purpose, I know not; but am rather inclined to believe it more accidental than otherwise, for reasons hereafter.

But, as I before observed, it will be found that the permanent incisores and cuspidati, in either jaw, are contained in sockets, separate from, and within the infantile or milk-teeth; and that as soon as they begin to enlarge, and rise a little in their respective sockets, this acrid fluid begins its operation, by destroying the partition bone, which separates the adult from the infantile tooth, and at the point which is the thinnest. As soon as it finds access to the root of the *Dent du lait*, it begins its operation upon it, and by slow gradation dissolves, sometimes a part, and sometimes the whole of the root of the infantile tooth, together with the infant alveolus, as may be seen by removing the infant teeth in a dead subject; where it will be found, that the attack on the root of the tooth is directly opposite and proportionate to the orifice, or loss of substance, in the bony partition that separates the two sockets, or alveolus. Here, Sir, is a real dissolution of the hardest and most compact bone in the human body, (though not by the absorbents, as some will have it) and a real absorption of those bones, when dissolved, no one can possibly doubt; for it has no other possible mode of escape. Here is that invisible agent, which has been a stumbling-block to many, in endeavoring to trace the cause of the destruction of the roots of the infantile teeth; and led some authors to declare to the world, that the *infant teeth had not nor ever had long roots or fangs*.

Having, I think, adduced satisfactory evidence of the existence of this morbid, or acrid secretion, and of its being the cause of the variety of unpleasant symptoms in infantile dentition; and, when possessing still more acrimony, is ca-

pable of liquifying our very bones, and, by the assistance of other agents, (the absorbents) of carrying them off; let me pursue it a little further. There is such a remarkable coincidence, or relation, between certain diseases, that the cause of one may often be suspected, and combated, while the legitimate cause remains unnoticed. In order to come at truth, in a point that may appear to be obscure, I know of no better method than that of reasoning from the known to the unknown. Improving this position in the present case, I must beg permission to step over the bounds of my small province, to examine some of the diseases of the general system, in order to trace the ravages of this little Hydra; for it is *impossible* that he can be confined to the destruction only of our infantile teeth, or to the cells, or cavities, that first enveloped them.

We are told, by some pathologists, that inflammation is excited by an increased circulation, from *irritation*, external or internal, local or universal; that *immediate* cause of inflammation is *irritation*: hence, we must suppose that irritation produces inflammation, and that inflammation produces suppuration.

But what produces irritation in the first instance; for example, previous to a case terminating in suppuration? We are told, it is "*spasm*—that spasm and inflammation mutually produce each other." This seems to me to be a very unsatisfactory definition of the true cause of either irritation or inflammation. But others, more liberal in their sentiments, give us, no doubt, a more plausible reason, by saying that the fluids, from various causes, are urged from their natural course, or channel; that as soon as this is effected, if not taken up by the absorbents, they either ferment or stagnate, and from the acrid quality which they acquire, they become capable of exciting irritation and inflammation.

This, I think, applies immediately to a diseased state of the gums in laborious dentition, and to the destruction of the infant teeth and their processes. Is this one of his employments, to produce irritation, inflammation, &c. in any case throughout the system, from the crown of the head to the sole of the foot, act upon our vitals, intestines, and to dissolve our very bones? No doubt, you will say No: But permit me, if I have not exhausted your patience, to follow him a little farther. Mr. Pott, tells us, that a particular distortion of the spine, which he describes, (and of which he

has given some beautiful engravings, representing one or more of the vertebræ, as it were, melted down) is owing to a *disease in the bone* itself, or to a *scrophulous* affection.— (In speaking of this disease, I do not mean to include the rachitis or necrosis.) If it is a scrophulous affection, why is its operation so partial, that is, confined, in most instances, to the dorsal or lumbar vertebræ? And how does it happen that (if scrophulous) a spontaneous cure of the above mentioned case is so often effected? We see persons frequently who have been subject, in early life, to this unpleasant infirmity or disease, some of whom have never received any medical assistance; and yet, they have recovered, are in perfect health, and all their limbs possess their just proportion and vigour. Are those cases of real scrophula, or are they the effects arising out of an accidental cause? I am much inclined to believe that they are oftener accidental than otherwise. Mr. Pott has wisely recommended (in this case) an issue, to be made on each side of the tumor, and to be kept open by powder of cantharides, &c. And what is its operation? To me, it appears to be the happiest and most appropriate that could possibly be applied; acting by derivation, or contra discharge, giving a different course to the fluids, and lessening this morbid or acrid secretion, in the same manner as setons in the back of the neck for high inflammatory diseases of the eyes, which have been practised with success. In the case of distortion of the spine, the bones appear to be liquefied, and carried off without pain in the part diseased, in the same manner as the teeth and alvoli in the infantile state; and are alike accidental—for if this acrid fluid was intended as a provision of nature, to carry off the infantile teeth, we have every reason to suppose that it would have been effectual, which, in the present instance, is not the case. If the adult teeth are placed somewhat more than usual within the infant incisores and cuspidati, the adult, or permanent teeth, are often thrown out of their proper direction, and retain this position, because the partition plate between the infant and adult teeth, still remains undiminished, and also the root of the infant or milk-tooth, which, when extracted, discovers no trace of the operation of this acrid fluid. Beside, the last infant molar tooth, in the superior jaw, has (like the adult molares) three roots, somewhat diverging, so that they are larger at their extremities, from one to the other, than the diameter of the

tooth at the crown. When the second bicuspid is protruded, which occupies the place of the molares, its diameter is less than that of the molares, consequently can only embrace the two external roots of the molar tooth, while the internal root, that traverses the roof of the mouth, remains in its process or socket, entire, without being acted on, or touched at all, by the fluid that surrounded the bicuspid, and destroyed the two external roots complete. Those internal roots are often retained in the mouth untill 18 and 20 years of age, unless sooner extracted.

How far this agent is concerned in the disease of the spine, I leave you, Sir, to judge, while I pursue him further. We are told, that the growth and pressure of tumors, (humoral, I presume) are capable of hurting the periostrum, destroying the bone, causing exfoliation, &c. (*see Bell.*) If we consider the name only, and admit any secretion from the tumor, we may readily grant that the periosteum is hurt, inflammation produced, and that the bones *are* destroyed from what is too often seen of their pernicious effects. But was there ever a *wen*, of whatever size or proportion, bound down upon a man's head by his hat, as is the case, (within my knowledge) in this city, where there are two or three of the size of an egg, ever produce any injury to the bone?—This I am unable to answer.

In the next and last place; if I can find no traces of this monster, I will readily relinquish the pursuit: but, "be the result what it may, I think, as the game is started," I feel a strong propensity to pursue it, and, if possible, to find out its lurking holes.

Let us see what he has to do with that dreadful disease, the aneurism of the aorta, and of the femoral artery, &c. by which the sternum, the costæ or ribs, and femur, are *beaten away*, and carried off, as we are informed. Permit me to examine a little into coincidence, or relationship, or what it may best please to term it:—We see that there are different muscles in the body, acting almost constantly, and sometimes with great violence, across certain bones, untill they are evidently rendered smooth by their action; yet, did they ever produce a dissolution of the bone in a healthy state? See what pathologists say, speaking of the true aneurism: "As the aneurismal tumor increases in size, it meets with resistance from the neighboring; and as the coats will be more or less affected, according to the degree of re-

sistance ; in some places they will be simply distended ; in others, *absolutely destroyed* : for instance, where the aneurism presses against the diaphragm, it will be thinner than where it suffers no pressure ; it is still thinner where it presses against the *tendinous part* of the diaphragm ; and where it presses the spine, it is the soonest corroded through ; a proof that all pressure must be avoided in all instances of this sort."—(*See Lara.*)

Is this true, that the coats of the artery, or aneurismal tumour, will not bear the pressure of the tendinous part of the diaphragm, and yet be capable of beating away our bones ? If so, I confess it overthrows a very general maxim, particularly among seafaring men, that "the hardest will fend off." Suppose that an aneurism exists in the femoral artery, and that it has occasioned a dissolution or partial injury to the bone, and the tumour is removed, and the artery taken up and secured in the best manner ; does the dissolution of the bone still go on ? Does not a partial injury to the bone sometimes heal spontaneously, after the removal of the tumour ? If so, is it not reasonable to conclude that this deperdition of the substance of the bone was occasioned by the action of some agent supplied from the tumour, and possessing a solvent power ? As before the tumour is removed, and the artery secured in the best manner, is not the systole, or impulse of the blood at that part, infinitely more violent than when it was unobstructed, or mostly so, and a reaction proportionate to it, until the anastomosing branches are capable of receiving the blood, and relieving it ? Does the dissolution of the bone still go on, in proportion to the increased violence of the blood acting against the ligature ? or does it even beat away the ligature ? If so, where is the use of dissecting the tumour ?—Farther—The pathologists say, (in speaking of the blood passing through an aneurismal tumour) that "notwithstanding this blood is fluid, *its passage* in the tumour is *retarded*," which occasions a *remissness* in its "*motion*." I believe so : as well might we expect (if I may make the comparison) that the general current or the Atlantic channel should flow, with the same degree of regularity, all the way to the bottom of the bay of Mexico, as that the current of blood should flow through a large tumour with the same regularity, as through its regular channel or passage. "This remissness," (says Lara) "in its motion, which is

more or less considerable, according to the size of the aneurism, occasions some of the fibrous parts of the blood to separate from the red part, and, adhering to the internal coat of the aneurism, it there forms fibrous strata, which may easily be taken for real membranes by those not accustomed to observe them. This fibrous stratum cannot be dispersed by any means, either external or internal; and pressure cannot be used, because thereby the coats of the artery are destroyed." Here we see again the coats of an artery will not bear pressure; and yet they are capable of beating away the sternum, the ribs, the os femoris, &c. and liquefying them, that they may be rendered susceptible of being carried off by the absorbents. This doctrine appears, to my feeble understanding, to be totally irreconcilable, either on the principles of reason or analogy. It appears evident that there is a diseased action in the coats of the tumour; and that, from the remissness in the action of the blood in the tumour, the degree of this disease must be increased almost in proportion as the tumour is enlarged. Will it be hazarding much by saying, that from this seat of disease there is a constant secretion of fluid, whose acrid quality is such as to erode, or destroy bone? It may be asked, why its operation is not the same on every side of the tumour, on the flesh, as well as the bone? It is very well known that any of the diluted acids will operate on, or dissolve bone, when they will have no effect on the skin or flesh; and that a wound, not too highly inflamed, may be washed with them without injury. In this instance it is possible that this morbid secretion may be reabsorbed, except when it comes in contact with the bone; which, not possessing the power, at least in so great a degree, is subject to its action, and consequently destroyed. In this last disease, as well as a gibbosity, or distortion of the spine, and also in the other cases that have been mentioned, there is, to me, such a similarity in their operations, with those cases of the teeth, and their processes, that it seems difficult to separate them—to me, at least, *impossible*. That a disease, which prevails in any part of the human body or limbs, particularly the bones, should manifest itself in the mouth, is not to be wondered at, when we may venture to assert, that there is not a disease, incident to the bones of the human system, that is not at times manifested in the mouth, independent of the teeth, *with all its characters*; some of which, if not properly attended to, are equally fatal to

the welfare and happiness of the patient, as if they had happened in the spine, or any other part of the body. Is it not more than probable, then, that the same acrid fluid which destroys the roots and sockets of the infant teeth, is the principal agent by which our bones are liquefied in cases of disease, as when the ribs and sternum are melted down, and carried off by the absorbents? I say melted down: I do not mean to infer from this, that there is such a degree of heat prevailing as to be capable of dissolving or softening the bone; it seems to be rather a chemical process, operating by slow degrees, proportionate, perhaps, to the high morbid state of this destructive agent, and also of the bone on which it acts—For instance, the body of one of the vertebral bones is spongy and soft, when compared to the os femoris. But when the properties of this acrid fluid are better understood, and we consider the component parts of bone, we shall be better able to determine their affinity, attraction, and operation. But I believe its operation on the infant alveolus and teeth, where there is a considerable loss of substance, and no remains of it to be found, to be sufficiently ascertained to need no further proof. The operations in all the cases above mentioned bearing so near a resemblance to each other, I am, perhaps too much, inclined to believe the effects are produced from one and the same cause.

REVIEW.

TRANSACTIONS of the AMERICAN PHILOSOPHICAL SOCIETY, held at Philadelphia, for promoting Useful Knowledge. Vol. VI. Part II. 4to. pp. 253, with plates. Philadelphia. Conrad & Co.

REGULARLY, since the commencement of our work, have we noticed the publications of this learned and respectable society. The first part of their present volume was published in 1803, and reviewed in the second volume of our second Hexade, (vol. 8th, from the beginning) p. 164 and 291.

The papers in this publication are thirty-two in number. We shall notice them according to the subjects on which they treat.

I. On ASTRONOMY. Under this head, the communications of Jose Joachim de Ferrer, Esq. the Spanish Astronomer, who has passed much time in our country, hold a conspicuous place. 1. *A Memoir on the occultation of Aldebaran by the Moon, on the 21st Oct. 1793; with calculations to find the longitude of Porto-Rico W. from Paris; the mean of which, he ascertains to be 4h. 33' 51" 9.—* 2. *The geographical position of sundry places in North-America, and in the West-Indies; calculated from astronomical observations.* The principal materials of this article are, an occultation of Jupiter himself, and of his first satellite, by the moon; the passage of Mercury over the disk of the sun, May 7th, 1799; egress of the same planet; and four observations made in the island of Leon. 3. *Observations on the eclipse of the sun, June 16, 1806, made at Kinderhook, in the state of New-York.* Our readers will recollect that we gave a description of this sublime and awful event, Med. Rep. Hex. 2. Vol. IV. p. 77, and ib. p. 200, soon after its occurrence. On the 8th June, Mr. Ferrer sailed up the Hudson, to the south landing of Kinderhook, well equipped with the apparatus to observe the eclipse at a place where it would be total and nearly central. He arrived on the 10th, accompanied by that distinguished English astronomer Mr. John Garnett. Among other results from this expedition, the latitudes of several places along the

Hudson were determined: as, for instance, that of New-York, at Partition-street, $40^{\circ} 42' 42''$; Newburgh, $41^{\circ} 30' 20''$; R. R. Livingston's house at Claremont, $42^{\circ} 04' 39''$; place of observation at Kinderhook landing, $42^{\circ} 23' 03''$; and a middle part of Albany City, $42^{\circ} 38' 38''$ 1-2. 4. *Further observations on the same eclipse.*—This is an elaborate exhibition and comparison of observations on the subject, collected from different persons and quarters. 5. *Observations of the Comet which appeared in September 1807.* Of this celestial visitor we took notice in our Hex 2, Vol. V. p. 197. The present observations of Mr. F. were made in Cuba, and are accompanied with various other astronomical remarks. 6. *Notes, with corrections, to be applied to the geographical situations, mentioned in the first part of the present volume.*

Another contributor of astronomical intelligence, is our distinguished countryman Mr. Andrew Ellicot. His pieces are the following:—1. *Continuation of the astronomical observations made at Lancaster in Pennsylvania.* These are the sequel of communications under a similar title in the first part of the volume. 2. *Observations on the eclipse of the sun, June 16, 1806.* William Dunbar, Esq. of Natchez, has also furnished *Observations on the same eclipse*; as have also President M'Keen, of Bowdoin College, in the District of Maine; and Simeon De Witt, Esq. of Albany, Surveyor-General of New-York. Mr. Dunbar is likewise the author of a *Memoir, on finding the longitude from the moon's meridional altitude*; and of another *on the comet of 1807-8.*

II. On MATHEMATICS.—1. *A demonstration of a general mathematical theorem*; by Joseph Clay, Esq. This is offered as a proposition that had been offered by the late Mr. Simpson, some time before his death. 2. *The description and use of a new and simple nautical chart, for working the different problems in navigation; with examples of its application according to Mercator's sailing, and sailing by the arc of a great circle, with a demonstration of its principles*; by John Garnett, of New-Brunswick, New-Jersey. It is a sufficient commendation of this investigation, that the Society honored the author with an extra-magellanic premium of a gold medal, as an expression of their sense of its merit. 3. *A general method of finding the roots of numeral equations to any degree of exactness; with the application of logarithms to shorten the operation*; by the same. 4. *On the best angles for the sails of windmills*; by the same.

III. On ZOOLOGY.—1. *Facts and observations on the Beaver of North-America*; collected by Mr. John Heckewelder. The information herein contained was derived from three experienced beaver trappers, and contains curious particulars of this remarkable animal. 2. *An essay on the vermilion colour of the blood, and on the different colours of the metallic oxydes, with an application of these principles to the arts*; by Samuel F. Conover, M. D. The doctrine which this ingenious gentleman supports is this:—"From the experiments which I have made on *light*, and from those detailed by the great Newton, and other celebrated philosophers, on which we may rely, it appears, that *light* is a mixture of seven different coloured rays, of different refrangibilities and reflexibilities, and that we are indebted to the sun for all the light we enjoy; that heat is a simple elementary body, and a necessary constituent of this planet; that oxygene gas is a compound of *light*, *heat*, and *oxygene*, and that oxygene is held in its gaseous state by the means of *caloric*; all of which have been proved by numerous experiments made by Berthollet, Davy,* and other eminent chemists, which being conceded, renders it unnecessary to detail them here. It has also been proved, beyond the possibility of doubt, by the experiments of the most respectable chemists, that the blood contains *iron*. Hence, when atmospheric air is taken into the lungs, the oxygene gas is absorbed by the blood in its passage through the lungs during respiration; and from the great affinity of oxygene to the iron in the blood, it unites with that metal, and the *red ray*, a constituent of oxygene gas, (the most difficult of refrangibility) is absorbed at the same time by the iron and becomes fixed, which constitutes the red oxyde of iron, and illustrates, in a philosophical manner, the beautiful phenomenon of the vermilion colour of the blood; while the heat is set at liberty, and the other six constituent rays of light, either become fixed in the other parts of the blood, or are carried off in a latent state, by expiration; for it is an established principle in optics, 'that some rays enter into the combination of bodies, while others are reflected, and this in proportion to the greater or less affinity of the several rays with these bodies.'"

* With the exception that Mr. Davy makes to the existence of *caloric* altogether—The first evidence of the existence of matter is, that it has motion; all the experiments on heat prove its momentum, and consequently it has attached to it all the properties of matter.

Dr. C. next applies these principles to the arts, and endeavours to show that they may, in a very high degree, assist manufacturers of porcelain, china, glass, and all kinds of pottery, by instructing them how to *burn in* and fix the different colours according to their *different refrangibilities*: "That is to say, the degree of heat which would be necessary to fix permanently the *red colour*, would be a temperature so high, as to burn out and dissipate all the other colours, provided all the seven coloured oxydes were made from the same metal, and painted on a piece of porcelain; therefore, to avoid an error of this kind, the manufacturer would be obliged to burn in the *red colour* first, secondly the orange, thirdly the yellow, fourthly the green, fifthly the blue, sixthly the indigo, and seventhly and lastly the violet colour: for, by an attempt to burn in and fix the violet colour first, and afterwards to burn in the red, before the latter could be accomplished, the former would be dissipated. Therefore, it is necessary to know that the degree of heat, sufficient to produce the violet coloured oxide of gold, would be of so high a temperature as to drive off all colour from the red oxide of lead, and convert it into a white litharge: hence, when several colours are to be fixed in, or burnt on porcelain, at the same time, the different coloured oxides from the different metals should be selected, which would all bear the same degree of heat—Say 1300 degrees of Fahrenheit's thermometer, consequently no two oxides of different colours from the same metal would answer; therefore, a knowledge of these principles, and their application, would enable the manufacturer to adorn and beautify his wares, and to bring to greater perfection the different branches of the arts." 3. *Statement of the deaths, with the diseases and ages, in the City and Liberties of Philadelphia, from 2d January, 1807, to 1st January, 1809*; communicated by the Board of Health. From this valuable document many useful and curious inferences may be drawn. It is to be hoped annual reports of this kind may be continued.

IV. ON GEOLOGY.—1. *An appendix to a Memoir on the Mississippi, No. XXX. of the first part of the volume.* We refer our readers to the copious extract we made from the original memoir, in our Hex. 2, Vol. II. p. 292. In this postscript, the intelligent author endeavours to give a theory of the Mississippi, and of rivers in general, and, in executing this task, to show that the doctrines of the European philosophers are wrong. Mr. D. reasons powerfully against

the position, that spouting fluids, issuing from orifices with velocities in the ratio of the square roots of the respective columns, ought to be applied, without modification, to every motion of water. He denies the correctness of the application of Guglielmini, Varignon, and Mariotte, in their systems by hydraulics. He controverts the conclusions of Bellidor, Musschenbroeck, and 'SGravesande, on the same subject; and he gives examples wherein Maclaurin and Desaguliers were considerably out of their calculations, as to the quantity and movement of running water. He even considers Mr. Watt, and the chevalier Buat, as calculating the currents of great rivers upon very mistaken data. But for Mr. D's elucidation of this inquiry into the natural motion of fluids, as depending solely upon the declivity of the surface, with his explanatory figures, we must necessarily direct to the volume itself, p. 195. 2. *A description of a cave on Crooked Creek, with remarks and observations on nitre and gunpowder*; by Samuel Brown, M. D. of Lexington, Kentucky. Already, on more occasions than one, have we mentioned the process of extracting saltpetre from the limestone caverns—See our Hex. 1, Vol. VI. p. 364, and Hex. 2, Vol. III. p. 86; where an ample exhibition of the facts may be seen, as well as some speculations thereon. In addition to the information therein contained, we gratify ourselves by copying from Dr. B's memoir the following valuable passage:—"The quality of the nitre procured from the earth in calcareous caverns, is universally believed to be different from that which is found in the sand rocks. I have not been able to ascertain, with any degree of precision, the quantity annually manufactured in this state, nor the number of caverns which are known to contain it. I have, however, visited several of the most remarkable of them, and from the best information I could procure I have formed the following estimate.

The great cave on Crooked Creek, a branch	<i>lbs. nitre.</i>
of Rock Castle, supposed to contain - - -	1000000
Scott's cave, two miles distant from the great	
cave, - - - - -	200000
Davis's cave, six miles distant from the great	
cave, - - - - -	50000
Two other caves, within a mile of the great	
cave, - - - - -	20000
A cave on Rough Creek, a branch of Green	
River, - - - - -	10000

"Besides these, which I have had an opportunity of examining, I have heard of many others in various parts of the state; some of which are esteemed very rich in nitre, and are said to be of great extent.

"The great cave on Crooked Creek, in Madison county, is situated about 60 miles south-east of Lexington. It has two mouths, which are 646 yards distant from each other, and about 150 yards from a large creek, which winds round the hill through which the cave affords a commodious passage for horses and waggons. The general level of the floor of the cave is 80 feet above the creek. The average height of the arch is ten feet, though in many places it rises to fifty or sixty. The breadth of the passage is generally about forty feet; in some parts it is seventy or eighty feet. The floor has the appearance of a large public road, which has been much frequented. The ceiling is, in most places, smooth, with but few incrustations or stalactites. In some of the chambers, however, there are appearances of Gothic rudeness and irregularity which are truly sublime. When these vast chambers are sufficiently illuminated by the torches and lamps of the workmen, they present scenes so uncommon and so romantic, that the most stupid beholder cannot contemplate them without expressions of the greatest astonishment. During the winter season the effect of these scenes is greatly increased by a stream of water, which, issuing from a small opening in the arch of the cave, about twenty feet above the floor, and falling into a bason, occasions a noise, which, in these calm regions, can be heard at great distance, and echoing from arch to arch, fills the mind with the idea of some mighty cataract."

[The remainder in our next.]

Memoirs of the Connecticut Academy of Arts and Sciences.
Vol. I, part 1. New-Haven, 8vo, 1810, pp. 216.—
Steele & Co.

LONG since have we noticed, in our Reviews, the publications of the associations upon the plan of the present, in New-York, Massachusetts, and Pennsylvania. We now congratulate Connecticut, in having laid before the public a collection of tracts, written by some of her literary and scientific sons. The project for an institution which might unite the exertions of literary persons in Connecticut, for the promotion of useful knowledge, was formed in 1799. The first meetings were held in New-Haven.—The society was organized; rules were adopted for its government; and lastly, an act of incorporation was obtained from the legislature, during the autumnal sessions of that year. The academy, so constituted, holds an annual meeting, on the fourth Tuesday of October, for the choice of its officers, and stated meetings on the corresponding days of December, February, April, June, and August; that is, it meets six times in a year, or once in two months. A leading and favourite object with the founders, was to collect for publication a statistical account of Connecticut. For this purpose various measures have been concerted. Though this desirable end is not yet accomplished, we understand it is progressing, and that nearly thirty communications have been received in answer to the circular letter of inquiry sent forth in January, 1800. But the chief objects of the academy are of much wider extent; being directed to every method of improving the sciences, arts, and happiness of their country, so far as the general state of its concerns, and their own leisure, would permit.

The book now before us is the first publication of the united labours of these academicians. The memoirs are seventeen in number. Four of them are from the hand of NOAH WEBSTER, junior, Esquire. These are the following: 1. *A dissertation on the supposed change in the temperature of winter.* In this he combats the popular and fashionable opinion, that the winter temperature in northern latitudes has undergone a material change, so that it has become warmer in modern, than it had been in ancient times. The argument is principally directed against the

doctrine of Mr. Williams, the historian of Vermont. It is conducted with learned research and much ability. From an extensive and detailed inquiry into the facts, as they occur in Palestine, Italy, Constantinople, and the vicinity of the Black Sea, the Alps, Gaul, Greece, Spain, and Germany, Mr. Webster seems inclined to believe that nature is remarkably uniform in her operations; and such remarkable changes, as some persons have pretended, have, in reality, never taken place in the northern hemisphere. He presumes that the opinion of the increasing mildness of the winter, has arisen from the acceptance of the accounts of a few severe winters, as descriptions of ordinary ones; and from a proneness to draw general conclusions from particular facts. After an examination of the historical documents of Europe, Mr. Webster proceeds to inquire into those of America: and after a laborious and serious search into the authorities extant, he concludes that neither Kalm, Smith, Lahontan, Wood, Winthrop, Winslow, Higgeson, nor others, have afforded any solid reason to conclude that the seasons are, on the whole, milder than they formerly used to be. We insert the conclusions themselves, which the author deduces from his erudite and laboured investigations.

“It appears to me extremely unphilosophical to suppose any considerable change in the annual heat or cold of a particular country. We have no reason to suppose that the inclination of the earth’s axis to the plane of its orbit, has ever been varied; but strong evidence to the contrary. If this inclination has always been the same, it follows that the quantity of the solar rays, falling annually on a particular country, must have always been the same. Should these data be admitted, we are led to conclude that the general temperature of every climate, from the creation to to this day, has been the same, subject only to small annual variations, from the positions of the planets in regard to the earth, or the operations of the element of fire in the globe and its atmosphere.

“The real truth seems to be, that when a country is covered with forest, the vibrations in the temperature of the air and of the earth near the surface, are less numerous, and less considerable, than in an open country. Dr. Williams himself has furnished the data by which to determine this point. In 1791, he found an open field froze to the depth of three feet five inches; at the same time, in a

forest, he found the temperature of the earth, to be 39° by Fahrenheit, seven degrees above frost. This fact solves the question here discussed.

"While a country is covered with trees, the face of the earth is never swept by violent winds; the temperature of the air is more uniform, than in an open country; the earth is never froze in winter, nor scorched with heat in summer; and snow that falls in November usually lies till March or April, although the earth below is not froze, but gradually melts the snow and absorbs the water. On the other hand, an open country is exposed to violent winds and frequent great changes of weather. The earth in winter is usually froze into a solid mass, from one to three feet thick; great snows alternate with heavy rains; the earth which is covered with snow to-day, is to-morrow left bare; and an iron surface of this week, is, the next, converted into soft mud. Hence, probably, as much snow falls in an open country as in a forest; or if the clearing of a country converts more of the vapor into the water, yet it is liable also to more extreme cold, which preserves a balance in the temperature. That these are facts every man knows, who has observed the difference between the open country and the forest, in our old settlements; and Dr. Williams himself has given the results of meteorological observations which confirm them, and disprove the common theory of a moderation of cold. In page 50 of his history, he states the difference between the heat of the earth in an open field, and in the woods, during the summer; by which experiments, it is demonstrated, that from the latter part of May to the close of August, the open country sustains about ten degrees of heat, beyond that of the forest; the thermometer being sunk ten inches below the surface of the earth. At another time, he found the winter temperature of the earth in the forest to be 39°, while, in open field, the earth was froze. The vibrations therefore in the temperature of the earth, when cleared, are found to be much greater than when covered with wood. The differences, according to Dr. Williams, are as follows:

<i>Winter temperature of the earth in the woods in</i>		
<i>Vermont,</i>		39°
	<i>of the open field at frost,</i>	32°

Summer Temperature of the Earth.

		In an open field.	In the forest.	Difference.
<i>May</i>	23,	50°	46°	4
	28,	57	48	9
<i>June,</i>	15,	64	51	13
	27,	62	51	11
<i>July,</i>	16,	62	51	11
	30,	65 1-2	55 1-2	10
<i>August,</i>	15,	68	58	10
	31,	59 1-2	55	4 1-2
<i>Sept.</i>	15,	59 1-2	55	4 1-2
<i>October</i>	1,	59 1-2	55	4 1-2

"From these observations, it results that in winter the earth of the forest is seven degrees warmer than the open field; and in summer, it is, on an average, from May 23 to August 31, 9 1-4 degrees colder—and on an average, from May 23 to October 1, 8 1-4 degrees colder. That is, the vibrations in the forest temperature of the earth are between 39° and 58°—only 19 degrees of difference between winter and summer—while the vibrations in the temperature of the open country, are between 32, or frost, and 68—making a difference of 36° between winter and summer.

"The vibrations of the temperature of the air, are more considerable; but it is an unquestionable fact that they are much greater in an open country, than in a forest; and so far is it from truth, that the clearing and cultivation of our country, has moderated the rigour of our cold weather, that the cold of our winters, though less steady, has been most sensibly increased. There is not a greater amount of cold during the winter, but the cold at times is more severe than before our country was cleared. The difference is so sensible, as to be a subject of popular remark among aged people.

"Another effect of clearing the country, is, to distribute the cold of the year more unequally: hence fruits are more exposed to spring frosts. This is a most serious inconvenience in Europe, and is becoming so in America. The reason of variable and late springs is obvious. While the earth is covered with wood, it is never froze, but as soon as the snow is dissolved in spring, vegetation begins. In an open country, after the snow is melted, the earth is to

be thawed; and the heat of the air, for two or three weeks, is incessantly absorbed by the earth and water, while the frost is dissolving. Hence the heat of a warm day in spring is speedily absorbed, and cold succeeds. This alternation must continue, till the earth is warmed. If the winter temperature of the earth in a forest is 39° , and that of the open country 32° , we may easily conceive what an immense quantity of heat it must require to raise the temperature of the open field to that of the forest. It must demand nearly all the heat excited by the solar rays in April, so that in our open country, the earth is probably not warmer on the last of that month, than it was, when a forest, on the first of the month.

"It will be remarked that in discussing this question, I have admitted the fact assumed by my opposers, that there has been a clearing and cultivation of Palestine, since the settlement of the Jews in that country; and of Italy, since the days of Julius Cæsar. But I must not quit the subject, without contradicting the fact assumed. The reverse is the truth.

"When Joshua led the Israelites towards Palestine, that country was very populous, inhabited by various tribes of people, and containing large cities, whose enormous walls terrified the Israelites. Never has that country been so populous as in the few first centuries, after the Israelites took possession of it. The country therefore could not have been covered with wood, but every foot of cultivable land was occupied by husbandmen.

"Equally true is it, that the countries on the north of Syria were as populous in the days of Darius, as at any subsequent period. It was the case also in Italy, which was more populous at the Christian era, than it has been for the last fifteen centuries. In all these countries, therefore, no clearing of the lands can have taken place, to influence the climates, within the period in which a moderation of cold is supposed. Germany, on the north of Italy, has been, in a degree, cleared; but the Rhetian Alps intervene between Italy and Germany; and the cold winds which affect Italy in winter, blow from those high lands, where the air is colder than in the less hilly country on the north. In every point, therefore, the hypothesis of a moderation of climate appears to be unsupported.

"I would only further observe, that if the cold has abated ten or twelve degrees in our climate, within a century

and a half, it must have been intolerable before that period. The mean temperature of Vermont now, is about 43° . If we deduct 10° only for abatement of cold, the water in deep wells in Vermont, two hundred years ago, must have been of 33° of temperature, or nearly at the freezing point; in Canada it must have been at 32° , or the state of congelation. If we suppose the winter only to have changed, and deduct one half the supposed abatement, still the result forbids us to believe the hypothesis. If we suppose the heat of summer to have lessened in the same proportion, as just philosophy requires us to do, the summers formerly must have been intolerable; no animal could have subsisted under ten degrees of heat beyond our present summer temperature. On whichever side we turn our eyes, we meet with insurmountable difficulties.

“From all I can discover, in regard to the seasons, in ancient and modern times, I see no reason to conclude with Dr. Williams, that the heat of the earth is increasing. It appears that all the alterations in a country, in consequence of clearing and cultivation, result only in making a different distribution of heat and cold, moisture and dry weather, among the several seasons. The clearing of lands opens them to the sun, their moisture is exhaled, they are more heated in summer, but more cold in winter near the surface; the temperature becomes unsteady, and the seasons irregular. This is the fact. A smaller degree of cold, if steady, will longer preserve snow and ice, than a greater degree, under frequent changes. Hence we solve the phenomenon, of more constant ice and snow in the early ages; which I believe to have been the case. It was not the *degree*, but the *steadiness* of the cold which produced this effect. Every forest in America exhibits this phenomenon. We have, in the cultivated districts, deep snow to-day, and none to-morrow; but the same quantity of snow falling in the woods, lies there till spring. The same fact, on a larger scale, is observed in the ice of our rivers. This will explain all the appearances of the seasons, in ancient and modern times, without resorting to the unphilosophical hypothesis of a general increase of heat.”

To this is subjoined a paper of supplementary remarks, comprehending various additional authorities, ancient and modern, in favour of the position taken and defended by the author, which they who feel an interest in the controversy will do well to consult at large. 2. *Number of deaths in*

the Episcopal Church in New-York, in each month, for ten years, from January 1, 1786, to Dec. 31, 1795; taken from the Sexton's books. This is rather an account of the interments in the cemeteries of that religious society, during the aforesaid time. It appears that almost two thirds of the burials were of children two years old and under, and that these died in the months of July, August, September, and October. 3. *On the decomposition of White-Lead Paint.* This contains some queries and speculations on the changes which this saturnine preparation undergoes by exposure to the weather. 4. *On the origin of Mythology.* This dissertation evinces a large share of literary labour and research, and is at once a monument of the industry and ingenuity of the author. To those who are fond of these discussions, and particularly such as have perused Bryant's Analysis of Ancient Mythology, and Faber's Dissertation on the Mysteries of the Cabiri, we recommend the perusal of the present memoir. The profound acquaintance which it evinces, with the Gothic and Celtic etymologies, is such as we but rarely meet with; and the philologist, who rises from the perusal of Tooke's second volume of the Diversions of Purley, may promise himself excellent recreation in examining the radical disquisition of Webster.

Two tracts in this collection come from the pen of ELIZUR WRIGHT, Esq. 1. *A Dissertation on the production of Vapour; in which is attempted to explain some curious phenomena that attend its ascent.* The writer recapitulates several of the hypotheses that have been invented to explain the process of evaporation; such as comminution, capillary attraction and inflation of bubbles. All these, he considers as liable to fatal objections. He concludes, *that evaporation is a dissolution of water by fire.* (p. 72.) We, however, should question the correctness of this definition. The effect of fire upon mere water is, to convert it into transparent and elastic steam. Such a modification of water is employed for working machines and engines; but it immediately cools in the open air, and condenses to cloud or visible vapour. We question also the propriety of confounding a dissolution with a solution. The latter of these processes is exemplified by the melting of salt in water, and the incorporation of water with air. Both substances combine with any decomposition of either. These are examples of *solutions*; but *dissolutions* are very different operations. They commonly take place when metals are acted upon by

acids and by water: then a decomposition either of the metal, if it is a compound, or of the menstruum in which it is immersed, is effected; and this severing of either, or of both the ingredients, into their elementary parts, is called *dissolution*. A full and comprehensive theory of evaporation, by the solution of water in atmospheric air, is contained in Hugh Hamilton's ingenious and too much neglected *Essays on Chemistry*; as well as in James Hutton's *Theory of Rain*. (1 Edinb. Phil. Trans.) 2. *Description of an Air-Pump; invented by Elizur Wright, Esq.* This is an ingenious piece of philosophical apparatus; but in order to be understood, requires the *figure* in the print, and the explanation thereunto annexed.

Professor Silliman is another of the contributors to this collection. His papers are two:—1. *A sketch of the Mineralogy of the town of New-Haven*. From this instructive piece of topography we learn, that amidst the primitive rocks composing the region east of the river Hudson, there is a district of secondary and alluvial materials, at and around New-Haven. He supposes the plain country in that vicinity, is a deposition by alluvium. He considers the mountainous and hilly country thereabout, as being composed of trap, whinstone or basalt, especially the East Rock, the West Rock, and the Pine Rock; and the adjacent eminences, in the same chain or ridge with the East Rock, are of the granitical order, consisting of feldspath and quartz, without mica, and with but little hornblende. The basaltic ridges are underlaid with horizontal beds of sandstone, as they are on the west bank of the Hudson, all the way from Wiehock to Haverstraw. The ensuing quotation will at once give an idea of Professor S.'s powers of description, and of the close resemblance there is between the strata of trap north of New-York, and those west of New-Haven.

"If there be any difference in the appearance of the whin of the West and of the East Rock, it may perhaps be said that the former contains more hornblende, and is more inclined to break into the rhomboidal prism. The tendency of whin stone to assume these regular forms, contributes very much to the utility of this stone, which thus presents fair faces for walls, and is easily made to tally with contiguous stones.

We cannot leave the whin stone mountains, without adverting to the enormous accumulation of the fragments of their columns, which is found at the foot of all of them,

which the writer has seen, either here or in Scotland. These fragments, which have every possible size, from a few grains weight up to a hundred tons, very naturally result from the innumerable seams which divide even the firmest whin stone rocks, into what may be considered as a collection of columns, standing side by side, and so contiguous, as, on the whole, to form one solid mass. Other fractures run at right angles to these, in such a manner as to cut off the perpendicular columns into blocks of various lengths. It happens, therefore, that whenever the tops of these columns become exposed to the atmosphere, in consequence of the washing away of the less consolidated matters which cover them, they become peculiarly liable to break off by the action of the weather. This occurs particularly from frost. The water insinuates itself into the crevices, and when it freezes, it happens, in consequence of the well known expansion of water during its congelation, that the columns become strained, and have a tendency to separate, whenever the cohesive force of the ice is diminished; therefore, especially in the spring, when the ice thaws, not only small masses, but even large columns, break off by their own weight, and fall to the bottom. At the *West Rock* particularly, one may see enormous masses which have fallen in this way; and such is the accumulation which time has produced there, that a sloping mass of ruins now extends more than half way up the mountain, affording strong confirmation of what was advanced in the early part of this essay, concerning the gradual demolition of hills and mountains by the action of the elements.

"The subject of the whin stone mountains (already extended perhaps too far) shall now be dismissed, with the single remark, that the columns so often alluded to, are not always perpendicular; sometimes they are greatly inclined, a remarkable instance of which occurs at the Hartford and Cheshire turnpike roads, near Mr. Whitney's, where the columns do not form an angle of more than 8° or 10° with the horizon, and rest upon a stratum of sand stone, having the same inclination in degree and in direction, which is south-west.

"From the *West Rock*, we bend our course westward and southward, along the brow of the hills, which now assume only a very moderate elevation. Frequent masses of granite, whin stone, quartz, and sand stone, accompany us

along through Westfield, till we arrive within a quarter of a mile of the Derby turnpike, when a new species of stone presents itself, and very soon becomes the predominate stone of the country. Its colour is bluish, inclining to white; its fracture hackly; its hardness is such that it may be scratched even by the nail. Its structure is schistose, the laminae are often variously contorted, and frequently striated, with laminae of quartz, and sometimes of mica, so that in many places it may be called *micaceous schistus*, and from its soapy feel it may generally be denominated *magnesian schistus*.

"There are considerable varieties in its appearance; sometimes it inclines towards argillaceous schistus, or slate, but is distinguished from it by its soapy feel, and, other times, it approaches the character of serpentine. Here and there, in this quarter, may be seen detached masses of porphyry, which seems capable of receiving a handsome polish, but no bed of it was observed, although it is more than probable it exists in the adjacent hills in considerable quantity.

"Nothing occurs to detain us in passing over the hills which lie between the Derby turnpike and those heights which overlook West-Haven, about midway between the Stratford road and the Sound. Insulated blocks of granite, whin stone, porphyry, and quartz, are scattered every where along, but the magnesian schistus is predominant, and from the heights just now mentioned to where they terminate in the flat ground, adjacent to the shore, we find nothing but immense strata of this *magnesian schistus*, rising every where to view, and discovering, whenever the road, a water channel, or a side-hill gives a view of the strata, an unvarying inclination to the west and north, forming an angle of perhaps 35° with the horizon.

"In some instances this schistus is sprinkled with beautiful spangles of golden coloured mica, which are very brilliant in the sun."

This is part of the region, in Connecticut, marked as secondary in Mr. McClure's Geological Chart, and described as reaching along the margin of Long-Island Sound, from Connecticut river to New-Haven.

2. *An account of the Meteor which burst over Weston, in Connecticut, in December 1807, and of the falling of stones on that occasion; with a chemical analysis of the stones.* In the composition of this memoir he was assisted by his colleague, Professor Kingsley. The paper is the same that

we inserted in our work, very soon after its original appearance; except that since that time it has been corrected and enlarged. The form in which it here appears, is the same with that which it bears in the Transactions, vol. vi. part 2. of the Philosophical Society of Philadelphia. With our review of that publication, this curious tract will, in due time, be further noticed.

Colonel Jared Mansfield, Surveyor-General of the United States, is the author of three communications. 1. *A calculation of the orbit of the Comet which lately appeared; together with some general observations on Comets.* 2. *On the figure of the Earth.* 3. *Observations on the duplication of the cube, and the tri-section of an angle.*

Two of the papers in this collection are productions of Jeremiah Day, Esq. Professor of Mathematics and Natural Philosophy in Yale-College. 1. *A statement of the quantity of rain which falls on different days of the moon.* As far as conclusions can be drawn from these observations, made during the years 1804-5-6 & 7, on the quantities of rain which fell at New-Haven, in different periods of the moon, it appears, that if a comparison be made between the quantity of water collected at the new moon, the full, and the quarters, the smallest proportion has fallen at the new moon, and the greatest at the quarters. 2. *A view of the theories which have been proposed to explain the origin of meteoric stones.* He reviews the several hypotheses, of their formation from materials raised into the upper regions of the atmosphere by exhalation; of their projection from volcanoes; and of their descent from the moon; and considers them all as embarrassed with the most serious difficulties. Prof. D. however, pleases himself with the notion of terrestrial comets, as proposed by the late President Clap, of Yale-College. The theory offered by that ingenious gentleman, Professor D. supposes, may be so modified as to suit the case of atmospheric stones. Of the correctness of this, we entertain, notwithstanding, as strong doubts as of any of the others. The reasons are stated in our present volume, p. 262. A citation of the passage will, however, introduce the ingenious Professor to discourse for himself.

"There is one other hypothesis, which, though not entirely without difficulties, appears to be encumbered with fewer than any other which has been offered to the public. Among the manuscripts of the Rev. Thomas Clap, formerly President of Yale College, was found a paper, containing

"Conjectures on the nature and motion of Meteors." This was published, some years after his death. It is thought, that the theory of "Terrestrial Comets," which it proposes, may be so modified, as to suit the case of atmospheric stones.

"The solar comets, it is well known, revolve round the sun in very eccentric orbits. In one part of their revolution they sometimes come so near as almost to strike his body. They then move off, far beyond the orbits of all the planets; and, in some instances, are gone hundreds of years before they return. President Clap supposes, that the earth is furnished with its system of comets, as well as the sun—that their size, and the period of their revolutions, are proportioned to the comparative smallness of the primary body about which they revolve—that, like the solar comet, they fly off, in very elliptical orbits; and, during the greater part of their circuit, are too far distant to be visible—that, in their approach to the earth, they fall within our atmosphere—that, by the friction of the air, they are heated and highly electrified—that the electricity is discharged with a very violent report—that they then move off in their orbits, and, by their great velocity, are soon carried out of our sight.

"It does not appear that the learned author of this theory was apprised of the fact, that substances frequently fall from these bodies to the ground. But the scheme requires very little alteration to accommodate it to this circumstance. We have only to suppose, that, at the time of the explosion, pieces are broken off from the surface of the meteor; and that these fall to the earth, while the main body moves on in its orbit.

"The hypothesis, if admitted, will account for most of the phenomena attending the fall of atmospheric stones. The *velocity* of the meteor corresponds with the motion of a terrestrial comet, passing through the atmosphere in an elliptical orbit. A body moving near the earth, with a velocity less than 300 miles in a minute, must fall to its surface by the power of gravitation. If it move in a direction parallel to the horizon, more than 430 miles in a minute, it will fly off in the curve of an hyperbola; and will never return, unless disturbed in its motion by some other body besides the earth. Within these two limits of 300 miles on the one hand, and of 430 on the other, (some allowance being made for the resistance of the air, and the motion of the earth) the body will revolve in an ellipsis, returning in regular periods.

Now, the velocity of the meteors, which have been observed, has generally been estimated to be rather more than 300 miles in a minute. In some instances, it is perhaps too great to suffer the body ever to return. But, in most cases, it is calculated to be such as would be necessary, in describing the lower part of an elliptical orbit.

"The *direction* of the motion also, agrees with that of a revolving body; but not at all with that of a mass of matter, accumulated in the atmosphere, and falling, by its weight, to the earth. The *dimensions* of these meteors too, are such as to indicate that they move in orbits of their own; as they are manifestly too large to be formed in the air by an accumulation of gases, or to be thrown from a volcano or the moon. They appear to have about the same proportion to their central body, the earth, as the little planets lately discovered between the orbits of Mars and Jupiter have to the sun, about which they revolve.

"The theory last stated, though in the main adapted to the purposes for which it was proposed: yet, it must be acknowledged, is not entirely satisfactory in the explanation of one or two particulars. It assigns a reason for the ignition and explosion of the meteor, which is not, perhaps, fully warranted by any observations and experiments hitherto made. The stones, when they fall to the ground, are found to be hot. The body of the meteor itself has the appearance of fire. It is undoubtedly in a state of ignition, at least at the surface. Whence is this powerful heat derived? President Clap supposes it is produced by the friction of the air—that the body, moving with great rapidity through the atmosphere, is both heated and electrified—and that, when it is nearest the earth, the electricity is discharged, with an explosion as much greater than thunder, as the meteor is farther distant than the common region of the clouds. It is well known that *hard* substances may be electrified, and even set on fire, by rubbing them together. But farther proof is wanted, to make it evident, that a body may be made red hot, by the mere friction of the air; especially of air as greatly rarefied as it must be in that part of the atmosphere where the meteors move.*

* NOTE.—Since the discovery of Mr. Davy that the earths are metallic oxides, it has been suggested that the bases of magnesia and flint may originally exist in the meteor, in the state of PURE METAL; and that, when the body comes from some distant region of the heavens, into our atmosphere, a sudden and violent combustion is produced by the very strong affinity of these substances to oxygen.

"There is another circumstance which is left unexplained by this theory. In a few instances, particularly that at Sienna, the falling of stones is said to have been accompanied, or preceded, by an apparent burning of the clouds. If this is any thing more than an optical deception, it seems to indicate a collection of combustible materials in the air. This appearance of fire in the heavens has been too long before the falling of the stones, to be the *effect* of the passage of the meteor through the atmosphere.

"With the exception of these two difficulties, neither of which ought perhaps to be considered as insuperable, the theory, which refers the origin of meteoric stones to terrestrial comets, appears to be embarrassed with fewer objections than any of the others which have now been mentioned. None of them, however, can claim to be considered as any thing more than theories. They are not yet supported by direct and positive proof. The subject is involved in too much obscurity to admit of a complete elucidation at once. The inquiry has commenced with a number of suggestions, which may be true, but which must be left to be confirmed or refuted by subsequent observations. This is not unfrequently the course which scientific investigations must of necessity take. The first step towards an important discovery, is often an ingenious conjecture. This gives the lead to a train of inquiries, which finally succeed in unfolding the true principles of the subject. It must be granted, that but little progress has, as yet, been made, in explaining the origin, nature, and use, of the bodies from which the atmospheric stones proceed. But the facts that have been collected have awakened curiosity. The approach of these meteors will hereafter be noticed with uncommon interest. Observations of their motion will probably be made, with as much accuracy as the opportunities furnished by their sudden and unexpected appearance will admit. But whether the mysteries of the subject will be unveiled, upon a farther investigation, time must determine."

A paper on the American Cantharis, or Melœ Americana, is a valuable article in this book. Its author, Dr. NATHANIEL DWIGHT, one of our worthy correspondents, has confirmed by observation and experiment, the natural history and economical application of the black potatoe-bug, as given by Dr. Chapman in *Med. Repos. Hex.* 1, Vol II. p. 174, and added thereto original remarks made by himself at Farmington.

A species of earth, having some external resemblance to gypsum, is treated of by *John C. Smith, Esq. in a letter to the secretary, Simeon Baldwin, Esq. on the Whitten Plaster.* This is a stony or rocky material, dug from a quarry in Kent, near the borders of New-York. It is probably the Dolomite which abounds in the calcareous hills of Dutchess County, that divide the waters running into Long-Island Sound from those which empty into the Hudson. It is easily convertible to powder, and is reported to be a good manure for maize, and in certain soils, for clover.

A brief report of a trial at law, in which the influence of water, raised by a mill-dam, on the health of the inhabitants in the neighborhood, was considered; by David Daggett, Esq.; is inserted in this volume. The history of the suit is concisely this:—At New-Milford, in Connecticut, a dam had been constructed across the Housatonick river. The neighbouring inhabitants had suffered very much from remitting and intermitting fevers. They ascribed these distempers to the noxious influence of the mill-pond. Under this impression, a number of them went and destroyed a part of the dam, for the purpose of letting out the water. The proprietor prosecuted them in an action of trespass, for the recovery of damages. The defendants acknowledged that they had demolished the dam as stated; but justified the act on the ground of its being a public nuisance, and the cause of the distressing sickness which had for several years visited New-Milford. This plea brought up the subject on its merits before the court and jury; when, strange to tell, the testimony adduced, pro and con, was so multiplex and contradictory, that the question became more perplexed by investigation; and both the court and jury were so fully impressed with that idea, that they decided in favour of the plaintiff, and awarded damages accordingly; saying that they could not find it proved a nuisance.

The remaining paper is an *Observation of an Auroral appearance in the evening of the first day of August, 1783, at Durham; by the late Elizur Goodrich, D. D.* This is a minute and circumstantial record of a luminous appearance in the atmosphere, having more relation to the aurora borealis than to any other phenomenon in meteorology. It is by registering and collecting facts like these, that we shall, in process of time, be enabled to comprehend more completely the history of light and colours.

Three Dissertations on Boylston Prize Questions, for the years 1806 and 1807. By GEORGE CHEYNE SHATTUCK, M. D. 8vo, pp. 192, Boston, 1808.

IT is a pleasing subject of consideration to find men of liberal minds and independent fortunes bestowing legacies on seminaries of learning, in order to promote the interests of science by the diffusion of knowledge. The natural indolence of man, in the pursuits of literature, requires more than common incitement to rouse it into vigour. Such is that which proposes the discussion of prize-questions, in which fame, honour, and emolument, are alike concerned. But for this, we should probably never have seen the dissertations before us. And thus Mr. Ward Nicholas Boylston has the merit of having brought forth a display of the talents of Dr. Shattuck.

The decision on the merits of these dissertations was pronounced by a committee of nine physicians, appointed by the Corporation of Harvard College, in Cambridge; to which institution Mr. Boylston, in 1803, gave a sum to be annually applied for the purpose of rewarding the authors of the best dissertations, proposed by the College, on subjects connected with medical, anatomical, physical, or chemical researches. Dr. Shattuck obtained the prize in 1806, on the question relative to *Mortification*; and in 1807 he carried off both, being the second and third dissertations in the present volume. We cannot too much praise the activity and persevering enterprize of a young practitioner, who has so successfully employed the time which the practice of physic generally allows a beginner, in writing three dissertations, and obtaining the premiums offered for the best exercises on the subjects proposed.

We are pleased to see, in the public account of the adjudication, prefixed to this volume, that in 1806, Dr. James Mann, (whom we have had occasion to notice before, Hex. 2, vol. 2, p. 308) was the other successful candidate in 1806, on *Dysentery*.

These three dissertations of Dr. Shattuck do not possess equal merit. Though they make a handsome exhibition of the author's talents, they do not surpass the expectations unavoidably arising, when it is considered how grave a body of examiners made the adjudication in their favour. The subjects of the dissertations are the following:

1. "On the difference between mortification produced by an external cause, and that which is produced by constitutional defect, the diagnosticks and proper mode of treatment of such."

2. "On the structure and physiology of the skin, with a view to the diagnosticks and cure of diseases usually denominated cutaneous."

3. "On the causes, diagnosticks and cure, of biliary concretions."

Of these, we deem the second dissertation to be written with the most discernment and skill. All these subjects would have been better elucidated by a more appropriate division into chapters or sections, comprehending the different parts or heads therein treated of. The author, however, is, perhaps, free from censure on this account, as the haste with which the dissertations must have been written would not, probably, have allowed of more perspicuous arrangement.

Though we are disposed to treat this writer with the utmost respect, we shall mention, with all the forbearance that the nature of the subject requires, one or two oversights. On the subject of *Trichoma*, or *Plica Polonica*, he observes (p. 102) that it "is an endemic disease peculiar to Poland and the neighbouring countries. The causes which produce it have not yet been satisfactorily investigated.—The disease is said to be both contagious and congenital." Had Dr. S. perused the memoir of Mr. Boyer on this subject, he would have there seen (*Med. Repos. Hex.* 2, vol. 6, p. 293) that it was neither "contagious," nor "congenital," and that its causes have been "satisfactorily investigated." He might likewise have found (*ibid. Hex.* 1, vol. 3, p. 156) an instance of a woman roused, by flagellation or switching, from the same state of insensibility caused by laudanum, as that of the dog which he mentions. (p. 62.)

We are sorry to learn from the author of this dissertation that the destruction, by alkalies, of the septic acid, formed occasionally from the perspired fluids, is discarded by all the learned in the science of chemistry. A comfortable reflection, however, remains; for the practical artists continue to remove the sour and pestilential taints of cloathing and bedding, of shirts and sheets, by potash, soda, and their various purifying and neutralizing compounds, upon Dr. Mitchill's plan, without asking the opinions of learned chemists.

A mistake is also made with respect to the origin of a discrimination between different miasmata. Dr. Shattuck writes, (p. 79) "Whatever the constituent principles of the perspirable matter may be, it is certain, that by long retention upon the skin or upon cloathing, and exposure to a certain temperature, it may undergo the putrefactive fermentation, and thereby become a fruitful source of disease, constituting what Dr. Rush calls *idio-miasmatic* exhalations, the infection of typhus, camp, jail, or hospital fever."

Although Dr. Rush has adopted the nomenclature proposed by Dr. Miller, (Med. Repos. Hex. II. vol. 2, p. 362) yet we recollect, that in 1805, the former gave credit to the latter for the practical distinction between miasmata, arising from causes which he there describes. For the information of Dr. Shattuck, we will further state, that Dr. Miller considers miasmata as of two kinds, and designates them accordingly. The one he calls *koino-miasmata*, the other *idio-miasmata*.* The ambient air, contaminated by these miasmata, he calls *atmosphæra koino-miasmatica*, and *atmosphæra idio-miasmatica*. All the diseases produced from these causes are denominated miasmatic. Miasmata, arising from marshes, and large bodies of animal and vegetable decompositions, exposed to the rays of a powerful sun, exert their influence to a considerable distance around, and are often wafted by the wind from their place of origin. They operate upon a large scale, and produce the malignant, remittent, and intermittent fevers, which sometimes constitute plagues and malignant epidemics. These he calls *koino-miasmata*, as they arise from common masses of filth, have a wide extended operation, and affect the inhabitants of a place generally.

The *idio-miasmata* give origin to typhus. They are more contracted in their operation, and exert their influence within a small compass. These are the miasmata that infest the body, bedding, and the habitation of the sick person. Hence, nosologists have given typhus a wrong character in making it contagious. Keep the body clean, give the patient a free circulation of air, or remove him from the contaminated atmosphere, and the virulence will be destroyed. But this is not contagion. A foul state of the body and clothing, arising from the secretions and excretions of poverty-stricken persons, confined to dark unventilated

* These are derived from the Greek; the first from *Koinos*, common or public, and the second from *idios*, personal or private.

abodes, creates a poison, a little atmosphere which envelopes the individual, or family, and engenders typhus, which may be received by another within the same atmosphere. This atmosphere is the cause of the disease, and not the consequence. Hence we find no specific poison to renew the disease, if the *atmosphæra idio-miasmatica* is corrected by cleanliness and ventilation.

In employing the word caloric as an adjective, we believe Dr. Shattuck is rather singular; and his "*caloric atmosphere*," so often used in the first dissertation, is uncouth, and not warranted by usage.

We recommend, however, the perusal of Dr. Shattuck's dissertations to the inquisitive physicians of this country, and hold them up as examples of what industry may achieve when well directed.

The works of THOMAS SYDENHAM, M. D. on Acute and Chronic Diseases; with their histories and modes of cure: with notes, intended to accommodate them to the present state of Medicine, and to the climate and diseases of the United States. By Benjamin Rush, M. D. Professor of the Institutes and Practice of Medicine, and of Clinical Practice, in the University of Pennsylvania. Philadelphia. Benj. & Thos. Kite. 8vo. pp. 473. 1809.

AFTER the lapse of more than a century of enlightened inquiries into the principles and practice of medicine, and after the universal veneration expressed, by the medical world, for the writings of Sydenham, it is unnecessary for us to add our tribute of respect to his character. There is hardly any fact in the history of medicine that more forcibly proves the superiority of practical truth to speculative reasoning, than the credit still attached to the observations of this writer. His example affords a lesson to all who write on medical subjects in the hope of immortality, that they must not only observe the phenomena of diseases for themselves, but listen to the voice of nature with undivided attention. A comparison of the works of Sydenham and Brown, very clearly illustrates the distinction we endeavour to establish. The former, sustained by the truth and genuineness of his facts, is borne aloft through successive ages, and defies the scrutiny of time; while the latter, though qualified by his talents for the highest flights of speculation,

is dragged to the earth by the weight of the faults which his predecessor sagaciously avoided. We cannot open the volume of nature, or appeal to the evidence of facts, without undervaluing the one as much as we applaud the other.

In the work before us, Dr. Rush has the merit of pointing the attention of physicians to the excellence of Sydenham's writings, without affording the least sanction to his blemishes or defects. In the true spirit of medical criticism, Dr. R. adverts to his undue reliance on the salutary efforts of nature in the cure of diseases; to his erroneous belief in morbid matter as the cause of diseases, and in the groundless doctrines of *fermentation*, *ebullition*, *digestion*, *separation*, *despumation*, and *expulsion* of this matter, for the purpose of effecting their cure; to his mistaken confidence in the improvements of nosology, as a means of advancing medical science; to his misconception of the true nature and definition of disease; to his unjust preference of vegetable to the more active mineral remedies; to his improper attachment to specifics in the cure of gout; to his error concerning the contagiousness of the plague; to his unfounded belief in the efficacy of certain inert simples, and of compositions in which the ingredients were too indiscriminately accumulated; and, finally, to his dependence on the exclusive benefits of experience, and his rejection of reasoning in medicine.

While Dr. R. has thus proceeded, under the guidance of truth and sound principles, to develope with freedom and boldness the errors of Sydenham, which were chiefly those of the age in which he lived, ample justice is done to his unequalled histories of acute diseases, to his display of the laws of epidemics, to his intuitive sagacity in discerning the latent existence and influence of preceding diseases, disguised and entangled in succeeding ones, to his judgment and decision in the use of cool air and evacuants, to his felicity in discerning the precise time and manner of administering his remedies, and to the accommodation of his practice in the same disease to the varieties of different seasons.

It would afford us much gratification to proceed further in presenting the view which Dr. Rush has judiciously given of the great value and importance of Sydenham's works, and to point out particularly the merits of the notes, with which he has enriched the volume, and accommodated it to the present state of medicine, and to the climate and diseases of the United States. But our limits unavoidably re-

strain us on the present occasion. We cannot, however, forbear to remark the seasonableness of this re-publication, with its numerous modifications and additions, in relation to the existing state of medical principles and practice. That a great and interesting revolution has been effected, in this respect, within the last twenty years, is universally known. And there was a peculiar fitness in Dr. Rush's engaging in the present work, on account of the distinguished share he has had in bringing about that revolution. For the purpose of showing the extent and importance of this change in doctrine and practice, we beg leave to lay before our readers the following observations on the subject, from the *Edinburgh Medical and Surgical Journal*, vol. v. p. 85 and 86.

“Physicians seem now to be gradually recovering from those delusions with which a false theory, more than mistaken or unguarded observation had beset them. Indeed, we have witnessed so many signs of the decline of the stimulating method of cure in the greater number of acute diseases, and in some chronic, that we have long since ventured to proclaim its downfall and disgrace. And we have seen enough to persuade us, that this approaching revolution will tend to the improvement of practical medicine.

“The practice founded on the Brunonian doctrine of debility, excitement, and stimulus, has slain, we believe, in sincerity of heart, its thousands and ten thousands. This doctrine not only set all former observation and experience at defiance, but so strong a hold had it taken on the imagination of its votaries, that medicine appeared to them unsusceptible of farther improvement. We remember the horror and incredulity that was excited some years ago, by the evacuating system of Dr. Rush and of some West-India practitioners, in the yellow fever; and how a celebrated German professor, of the stimulating school, shrugged up his shoulders, and prognosticated the certain death of a patient in our infirmary, labouring under typhus, when he heard his physician prescribe some necessary evacuations: But the practice of Dr. Rush was successful; and our condemned patient speedily recovered. The most obvious and most fatal error of the doctrine now alluded to, was, that every evacuation was necessarily and directly debilitating, and that strength and excitement could only be effected by stimuli. In fevers, therefore, all evacuations were dreaded, while wine, brandy, and opium, were considered as sovereign remedies, and too often employed with little

discrimination, and still less regard to the state of local symptoms and organic affections. This fashion has passed away; and we now see physicians prescribing topical and general bleedings, drenching their patients with cold water, and scouring them with jalap and calomel, without at least any increase of the mortality of our fever wards.

“Our dread of debilitating by such evacuations has been greatly lessened by the example of foreign practitioners, and by the writings of some enlightened physicians of our own country, particularly of a Currie, Jackson, and Hamilton.

“These revolutions in the practice of medicine are calculated indeed to excite in our minds no inconsiderable degree of scepticism, and almost tempt us to conclude, with some, that nature triumphs more than art in the cure of diseases, at different times so oppositely treated. The truth is, that in fevers, there is no practice invariably proper; the physician must be guided by symptoms and circumstances; sometimes he must moderate by an antiphlogistic treatment the dangerous reaction of the vascular system, and sometimes he must support the vital energy by cordials and stimulants; but, at all times, he will find it useful to remove sources of morbid irritation.—We have no hesitation, therefore, in believing that physicians are in a much fairer way, as well as their patients, now that they have recovered from the delirium of Brunonian excitement.

“In the treatment of chronic diseases also, we have heard less of beef steaks, wine, and tonics, than we used formerly to do, and much more of temperance, exercise, and evacuation. The valuable observations of Dr. Hamilton on the exhibition of purgative medicines, seemed to lay open new views of the pathology of some of those chronical diseases, which might lead to improved modes of practice.”

We conclude by observing, that we feel the less regret at the brevity of our remarks on this work, in the assurance that its condensed and cheap form will carry it into the hands of every practitioner of physic throughout our extensive country.

MEDICAL AND PHILOSOPHICAL INTELLIGENCE.

INFORMATION *relative to the DISTEMPER in Worcester County, (Mass.) called the MALIGNANT or SPOTTED FEVER: In communications from Dr. RICHARD WAIT, of New-London, (Conn.) to Dr. MILLER, dated March 22, 1810.*

SIR,

I INCLOSE you a valuable document, and hope to see it embodied in that great national work, the Medical Repository. Originally appearing in a village gazette, it may have a limited circulation; and productions of merit, when committed to the light ephemeral sheet of news, hardly survive the revolutions of a year.

That singular and fatal disease, denominated *Spotted Fever*, is making its appearance in every part of our country, and it seems as if the prediction of an American poet, when describing the precursors of the final judgment day, was about to be verified:

In every blast the *Spotted Plague* be driven,
While angry *Metecors* shoot across the heaven.

From a Worcester, (Mass.) paper of the 14th instant, I transcribe the following:—"The remote causes of this disorder are said to be improper or putrid diet, *corrupted grain*, a too free use of those salts which dissolve the blood, putrid miasmata, &c. It has always raged in the winter, after seasons which are unfavorable to the productions of the earth. The corn was, in many places, killed by the frost last year, and of course it cannot be so wholesome as when it arrives to maturity. We have lately heard of several animals having been killed by eating *mouldy corn*; it is also destructive to human beings. European writers have given accounts of very fatal consequences which have resulted from blasted, spurred, or otherwise damaged rye."

I remember having read in the newspapers an account of a number of horses dying in Newark, (N. J.) a year or two ago, in consequence of eating mouldy corn.

MASSACHUSETTS MEDICAL SOCIETY.

At a meeting of the Counsellors of the Massachusetts Medical Society, on the 7th of February, 1810.

VOTED—That a committee be appointed to collect information respecting the history and treatment of a malignant disease, commonly called SPOTTED FEVER; which is now prevailing in the county of Worcester, and has prevailed, within five years past, in Hartford, Connecticut, and Providence, Rhode-Island, and report at the next meeting of the Society, or Counsellors.

DR. FISKE, } Committee.
DR. PAINE, }

From the Records,

JOHN C. WARREN, *Rec. Sec.*

N. B. Any communication upon the above subject to the committee, or to Dr. THOMAS WELCH, *Corresponding Secretary of the Medical Society*, at Boston, will be thankfully received.

March 1, 1810.

Letter from the Rev. FESTUS FOSTER to the Editor of the Worcester Spy.

PETERSHAM, March 6, 1810.

SIR,

I hasten to give you a sketch of the *Spotted Fever* in this place. It made its first appearance about the beginning of January last, but the instances were few and distant from each other, until last week. Although it had proved fatal in most instances, seven only had died, belonging to this town, previous to the 25th of February. Since that time, the disorder has come upon us like a flood of mighty waters. We have buried eight persons within the last eight days. About twelve or fifteen new cases appeared on Thursday last; many of them very suddenly and violent. This was the most melancholy and alarming day ever witnessed in this place. Seven or eight physicians were continually engaged in the neighborhood north of the meeting-house, and I believe not one half hour passed in the forenoon without presenting a new case. Pale fear and extreme anxiety were visible in every countenance. The faculty themselves felt their situation in being called to encounter an enemy with which they were little acquainted, and before which

their late brother in this place had so suddenly fallen.— They, however, manifested great skill and presence of mind, and, fortunately for us, adopted and strenuously pursued, that mode of treating this disease, which, they since learn, has best succeeded in other parts of the country. By these means, most of the patients whom they seasonably visited, are, by the blessing of a merciful God, yet alive, and hopefully on the recovery. The number of cases which now require medical assistance in this town, is about twenty, most of which are nigh the centre. I know of no case which is considered desperate. It is with pleasure I inform you, that we *hope*, though we dare not *believe*, that means have been, or soon will be discovered, to prevent the mortal effects of this malignant and alarming disease. The faculty have been so incessantly occupied, and so anxiously watching the success of their experiments, that they have not found leisure to make any communications to the public. So soon as their experiments shall be matured, they will, we presume, publish the characteristics of the disease, and the most effectual method of cure. It seems to be very generally agreed, that there ought to be no evacuations, but by external applications produce free perspiration, and internally apply the most powerful stimulants.

From the same to the same.

PETERSHAM, March 9, 1810.

SIR,

AT the request of a number of persons in this town, the gentlemen who have attended as physicians in the epidemic now prevalent among us, have made a general account of the symptoms and treatment of that most formidable disease. The success which has attended their practice, clearly evinces the general principles which they have embraced, to be correct. Should the disease continue to prevail, a further investigation may, probably, much improve their general system. The internal application of powerful stimulants was adopted, not from any previous knowledge that they had been tried and found successful, but because other means which had been employed proved ineffectual. We have been happy since to learn, that the method here pursued has been found the best remedy to this disease in Connecticut, and distant parts of this state. Believing that if the epidemic should appear in other parts

of the country, a knowledge of the method here pursued, and the consequent success, might be satisfactory, and possibly of the highest advantage to a distressed and afflicted people, you are requested to give publicity to the "Account" which I herewith transmit to you.

Some Account of the Symptoms and Treatment of a singular and fatal Disease, commonly called the Spotted Fever, which has made its appearance, and now rages in Petersham, in the county of Worcester, and some other adjacent towns, particularly the town of Dana.

The following are some of its characteristic marks, as it has fallen under our observation. It begins with shifting pains in different parts of the body, most frequently in some of the limbs, often in some of the larger joints, as the knee, the hip, or the shoulder, shifting from place to place, and frequently to the head or stomach, and often from the one to the other of these last-mentioned parts, with a sense of universal uneasiness or restlessness.

These symptoms are accompanied with cold shiverings, and other marks of fever, which are soon followed by a remarkable and general prostration of strength, and a depraved action of the sensitive organs. In some violent cases the sight is much impaired, and even totally, though temporarily, lost.

The eyes appear sometimes dead or glassy; but at other times, especially during the progress of the disease, they appear red or suffused. The pupil is frequently more or less dilated; but sometimes contracted to almost a point; and sometimes these states alternate with each other. The tongue has been invariably covered with a whitish coat, and moist.

The pulse is generally a little increased in frequency, remarkably intermittent, and between the intermissions unequal both in strength and quickness; but in some few mild cases it is very little altered.

There is generally great distress at the stomach, with nausea, and for the most part some vomiting.

Respiration is in all cases much disordered; but the labour seems to arise rather from the difficulty of inflating, than from any infarction of the lungs, as there is no cough.

Petechiæ, or livid blotches, or a red fiery eruption, some-

times in clusters, and sometimes in large and distinct pustules, in most cases, appear on the surface of some parts of the body, and sometimes they are general. These pustules most commonly break, discharge a little thin watery fluid, and then dry up; but sometimes they mature, forming ulcers, which may not heal till after recovering. But neither the spots nor the eruptions are inseparably connected with this disease. But when the eruptions do appear, they are attended with much itching.

Consciousness, especially in adult males, sometimes remains to the last unimpaired. But in females violent hysterical symptoms, with high delirium, have, within a few hours from the attack, supervened. And in young children a stupor sometimes comes on soon after the vomiting, which announces the approach of the disease, and continues till death.

In regard to the prognostics in this disease, our observation enables us to state, that from those cases attended with petechiæ, few recover; whilst those accompanied by an early eruption, more generally and more safely get through the disease.

The duration of the disease is to us uncertain. Some have died within twelve hours, others within twenty-four, from the time of the attack; while a large proportion of others have had the violence of it broken within forty-eight hours; when it run into the form of a mild typhus of uncertain duration.

In the treatment of this most formidable, and too often fatal, disease, the experience we have already had, warrants us in stating, that while its rapid progress, especially in its most violent forms, deprives us of all hopes of success, from the use of mild alterative medicines; so its peculiar nature, forcibly interdicts the employment of all drastic remedies, which may produce any great degree of depletion of the system; and thereby sink the patient irrecoverably. An injection of milk and molasses, with a teaspoonful of common salt dissolved in it, has induced strong spasms. An emetic of spruce with a grain of tartrite of antimony, operating only once upwards, and once downwards; likewise a dose of sulphate of soda, producing only two moderate operations downwards, have each of them, in hale young men, reduced the pulse from an hundred, down to forty-eight strokes in a minute. From these facts, it is easy to conceive how cautious we should be in the use

of emetics and cathartics in this very singular disease. If, however, at the beginning of the disease, there be a troublesome puking, a few grains of Ipecac. or warm water, may be drank to clear the stomach, and check that symptom; but an opiate should immediately follow the operation.

The only safe and efficacious mode of treatment, which has occurred to us, consists in the bold and liberal use of the diffusible stimuli, proportioned to the violence of the disease, together with the employment of the several means of powerfully determining to the surface, and in keeping up that determination in proportion to the exigency of the case, till relief shall have been obtained; and at the same time exciting the action of the brain, by blisters applied to the temples and nape of the neck, and by ether and other stimulants, applied to the head.

The diffusible stimuli employed by us, have consisted chiefly of brandy, opium, ether, volatile spirit, and camphor, and in some hysterical cases the pure oil of amber: and the means of determining to the surface, have been the employment of warm bath, followed by the assiduous application of stupes wrung out of a solution of salt in hot vinegar or water, and applied to the extremities and stomach, and kept warm by hot stones or bricks, or billets of wood, taken out of boiling water, and placed round the patient; together with a free use of an infusion of snake-root and saffron, or pennyroyal, with such of the above mentioned diffusible stimuli, as appear best adapted to the case, and frequently supped warm. By these means, a gentle and universal sweating is induced, and should be continued until the disorder gives way.

To what extent the most powerful of these diffusible stimuli may be safely and necessarily employed, the following facts will shew. A young woman, aged about 20 years, who recovered from the disorder, being very violently attacked, and a high delirium with great distress supervening, took more than a quart of brandy, and not less than twenty grains of good turkey opium, aided by the above means of determining to the surface, in less than twelve hours, and before any material mitigation of her disorder could be obtained, and what is truly wonderful, without the least appearance of intoxication. Indeed, we have been obliged frequently to exhibit ten grains of opium for a dose in some of the most violent cases, attended

with strong spasms, and have never known it to produce stupor in a single instance.

In one instance only have we employed the lancet. A man about twenty-eight years of age, having been violently attacked, and attended by strong spasms; on the third day, his spasms having been subdued, was exercised with extreme difficulty of breathing, and great oppression at the breast, and exhibiting at the same a strangulated countenance, which symptoms appearing to arise from a surcharge of the vessels of the lungs, owing to their inaction, rather than their inflammation; sixteen ounces of blood were taken away, merely to restore the equilibrium. This had the happiest effect in relieving those distressing symptoms. But these symptoms returning on the fourth day, the operation was repeated, and with the same salutary effects. The blood did not exhibit the usual marks of an inflammatory diathesis, but on standing, remained destitute of the inflammatory buff, and its coagulum was of a loose texture and tender.

Out of twenty-five cases, which have been subjected to the above mode of treatment, only three have as yet proved fatal, and those were patients under four years of age. The others are in a hopeful way of recovery.

Great alarm has been excited on account of the supposed infectious nature of this disease. But for the consolation of the people we can say, that from the most careful observation we have been able to make, we cannot discover a single instance, in which it clearly appears, that the disease has been communicated from one to another by contagion.

And as we are of the opinion, that none CAN be affected by this disease but those in whom a CERTAIN CHANGE in the state of the body, commonly called the state of PREDISPOSITION, has been affected by the influence of a certain morbid quality in the atmosphere, which state of predisposition disposes the moving powers, upon the application of any exciting morbid cause, to produce those specific morbid affections which characterize this particular disease; and as we are furthermore of the opinion, that the only rational means of checking the progress of this, or any other epidemic, consists in removing the state of predisposition; and as the strong impression made on the system by the application of cold water suddenly applied to the surface, appears to be one of the most probable means of destroying this state of predisposition, by dissevering the chain

connecting those morbid affections; we cannot but strongly recommend the adoption of the practice of the cold bath, either by plunging, showering, or affusion, which may be employed three successive mornings, (the weather being fair) then omitting it three, and thus proceeding to the third time, as a most probable mean of arresting the progress of this malignant disease, and of preventing its extending its ravages.

ABRAHAM HASKELL,
MASON SPOONER,
JACOB HOLMES.

Petersham, March 9, 1810.

THE "SPOTTED FEVER."

[The following Circular has been addressed to Medical and other gentlemen, near the places where this epidemic has lately prevailed: But, as there may be others who may be able to afford information on the subject, to whom it has not been sent, we readily comply with the desire of the committee of the Counsellors of the Massachusetts Medical Society, to insert it in the Chronicle; with the request that gentlemen, either of the profession, or others, who may have it in their power, will attend to the questions, and transmit their replies to Dr. Thomas Welsh, Corresponding Secretary of the Massachusetts Medical Society, without paying postage—and we are authorized to say, that the committee are happy in being able to state, from the communications they have already received, that the disease is, at this time, far from being a mortal one; and that it is considered by their correspondents, as being under the controul of medicine as much as ordinary diseases.]

BOSTON, March 30, 1810.

SIR,

In consequence of the alarming extension of the very fatal disease, which is commonly called the *Spotted Fever*, the Counsellors of the Massachusetts Medical Society, have considered it their duty to investigate with peculiar care, every thing relative to the causes, history, and modes of treatment of that disease. They have, therefore, at their

meeting on the 27th inst. appointed the subscribers a *committee* to correspond with, and collect materials from the fellows of the Society, and other gentlemen of character, in whose vicinity the disease has prevailed.

As one comprehended in this description, we take the liberty to address you; and, in behalf of the Massachusetts Medical Society, we beg leave to request you to answer all or any of the subjoined queries. We beg leave, at the same time, to request, that if in any instance your replies are not founded on your own personal observation, but on information from others, you would do us the favour to notice from what sources such information has been derived.

It is the wish of the Counsellors that we should make a report to them on this subject, so interesting to every member of the community, as soon as the circumstances can possibly admit. We shall therefore be peculiarly obliged to you, if you will seize the first moment of your leisure to prepare your answer. We are aware, that your important engagements at this time, must render a compliance with our request very inconvenient to you; and, did we not consider ourselves as acting in behalf of the whole community, we should not feel at liberty to make our call upon you so urgent. Should you be able at present to give very concise answers to a part only of the queries proposed, at a future day you might add whatever else shall have been furnished by subsequent experience and inquiry.

It will be most convenient both to yourselves and to us, if you will refer to the number of each question, without recapitulating it, and reply to each distinctly, in the order in which they are placed.

1. At what time, and at what place, did the first case of this disease occur under your observation?

2. What are the symptoms of this disease, the order of their occurrence, and the most usual duration of each?

3. Have you noticed any precursory symptoms by which the approach of the disease can with tolerable certainty be ascertained?

4. Is the disease to be distinguished only by attending to the combination and course of the symptoms, or by any one or more peculiar symptoms?

5. Have any persons previous to the attack of this disease been subjected to any extraordinary fatigue, or exposure to irregularities or peculiarities of diet, or to any other circumstances which can be considered as exciting causes; and if

any, in what proportion of cases have such circumstances been noticed?

6. Have you noticed any circumstances which lead you to consider the disease contagious or infectious: or have you noticed circumstances which lead to the opposite opinion?

7. Are persons of any particular class or profession, age, sex, or temperament, peculiarly subject to this disease; or have persons of any particular description been peculiarly exempt from its attacks?

8. Have any particular evacuations, or other symptoms, marked the crisis of this disease; and has it, in any cases under your observation, been followed by affections of the joints or glands, by erysipelas or any other local affection; and what has been the termination of such cases?

9. Are there any symptoms in the early stages, or in the course of the disease, on which a prognosis of the event may be founded; and with what degree of assurance may such prognosis be formed?

10. Are relapses frequent; and do the symptoms differ from those of the original attack, either in kind or degree?

11. Do those who survive the disease recover speedily; or is there noticed any remarkable change in their state of health?

12. What mode of treatment have you tried or seen tried; and what has been the success of each mode?

13. What have been the external appearances on examination of the body after death; and at what distance of time after death have your examinations been made?

14. What have been the internal appearances ascertained after death by dissection, and at what period?

15. Have you, at any former period, seen the same disease, or any nearly resembling it?

16. Have there been any unusual diseases, or any unusual mortality among brute animals in your neighborhood?

17. Are there any other points on which you can give information, which appears to you to be material at the present moment?

In reference to the second and fourth queries, we beg to call your attention particularly to the following objects:—1. Symptoms denoting local affection of the brain, such as delirium, or any peculiar affection of the mind, spasms and convulsions, remarkable insensibility, deafness, unusual dilatation or contraction of the pupils, paralysis of any part of

the body, &c. &c.—2. Symptoms denoting affection of the stomach, or any of the abdominal viscera, such as increased tenderness, fulness or tension of the abdomen or any region of it; vomiting or purging, the nature of the matters discharged either from the stomach or the bowels, &c. &c. In reference also to the 14th query, we wish to learn especially the appearances in the head and stomach.

It is unnecessary to suggest to you as being likewise objects of attention under the second query, the state of the skin as to temperature, dryness or moisture, and as to spots of different descriptions which may be remarked on it; the countenance, and especially the eyes; the respiration; the pulse, whether dissimilar in different parts of the body; the urine, &c. Nor can it be necessary to state the importance of noting the stages, or periods of the disease at which any observations are made, or any remedies are tried. We may, however, remark, that as violent diseases have sometimes grown more mild in the latter part of an epidemic season than in the beginning, it will be useful to note at what part of the season your various remedies have been employed.

There are some other questions which might be added to those above, particularly respecting the soil and other circumstances of the districts in which your observations have been made; respecting the state of weather before and during the prevalence of the present epidemic; the diseases which have prevailed during the year past, the direction in which the disease now prevailing has been extended, whether this has or has not corresponded with the prevailing course of the winds, or with the ordinary lines of intercourse; respecting any thing peculiar in the grain raised last year, &c. But probably your time is now too much occupied to attend fully to questions on these subjects; and with your permission we may perhaps address you respecting them at a future time.

Shall we, in addition to other requests, beg you to inform us, if possible, once a week, or as often as you can, what may be the progress of this lamentable disease, within the circle of your observation. Your letters may be addressed and sent to DR. THOMAS WELSH, in Boston, whenever a direct conveyance by mail or otherwise can be procured; in other cases they will reach us less directly, but very safely, if inclosed to DR. OLIVER FISK, at Worcester.

With the most sincere sympathy for the sufferings to which you, and your neighborhood are exposed; and with great respect, we are, Sir, your obedient servants.

THOMAS WELSH,
JAMES JACKSON, } *Committee.*
JOHN C. WARREN, }

STODDARD'S Sketches of Louisiana and Florida.

MAJOR AMOS STODDARD, of the United States' Artillerists, who has been stationed several years on the Mississippi and Red River, is about publishing *Sketches, Historical and Descriptive, of Louisiana*. This work will contain about five hundred pages, and embrace a concise history of Louisiana, from its first discovery to the cession; the boundaries and extent of it—some account of the Floridas—a description of what is called the Delta, including the City of New-Orleans—of the country between the Delta and the Arkansas—of the territory about Red River and to the westward of it—of Upper Louisiana, or all that tract, particularly the settled parts of it, to the north of the Arkansas, and also some sketches of the interior regions—an account of the population, climate, diseases, soil, productions, natural growth, &c. &c. of the country—the nature of the Spanish government and laws, and the mode of administering justice—of the lands and land titles—of trade, commerce, and manufactures—of learning and religion, and the manners and customs of the creole inhabitants—of the extraordinary appearances of salt, mines, and minerals—of the antiquities—of the aborigines in general, and of the probability of one or more Welsh tribes in America—a description of the large rivers, and some observations on the state of slavery in that country.

History of New Netherland.

AMONG the early historians of the country, called, at the time of its settlement, *New Netherland*, and since comprehending parts of New-York, New-Jersey, and Pennsylvania, ADRIAN VAN DER DONK, has acquired a large share

of reputation. He wrote a *geographical and natural history of the Dutch Colony on the North and South rivers*, in Low Dutch. It was published at Amsterdam, by Evert Nieuwenhoff, in the year 1655. This early and original account treats of the situation, discovery, name, and settlement of this colony of Hollanders. The boundaries, coasts, rivers, waters, forests, and face of the country, are described. The plants, animals, minerals, climate, and seasons, are duly noticed. To which is annexed a full and satisfactory description of the native inhabitants, including their manners, customs, policy, and opinions concerning a future state. It is with pleasure we announce, that an English translation of this curious work of Van Der Donk, has been undertaken by the Rev. JOHN BASSETT, of the Boght, near Albany. The work, we are assured, is in great forwardness, and in a very short time will be ready for publication. It must be quite a treat to our literary gentlemen, and especially to the learned members of the historical society, to read this celebrated treatise in their own tongue. While we express our sentiment of applause towards Dr. Bassett, for having bestowed a portion of his time and talent in the translation, we hope his version will soon be offered to the public, and that it will meet with a liberal and generous patronage.

TITFORD'S *American Hortus*.

A BOTANICAL work, with coloured figures, is offered for publication in New-York, by Mr. W. I. TITFORD. This gentleman, from a residence of many years in Jamaica, has enjoyed frequent opportunities of becoming acquainted with the plants of that luxuriant island. He proposes to delineate and describe the vegetables, not merely of Jamaica, but of the West-Indies, and of the tropical and continental regions in North and South-America.—He intends to comprise the whole in one volume quarto, under the title of *Hortus Botanicus Americanus*. The author promises also, an Essay on Tropical Climates, and the means of preserving health; with various other useful matters. It is intended to notice about five hundred plants; and of these two hundred will be figured, many of which, it is alledged, were never before engraved.

*Reasons for supposing that the great Lakes of North-America
were originally composed of salt water.*

(From Mitchill's Tour to Niagara.)

THE remains of marine animals throughout the continents of the earth, have long been cited as proofs that the Ocean formerly covered them. Lithophytous and testaceous substances are so numerous and distinguishable, that in the present cases the mind is easily led to this conclusion. They abound the greater part of the way from Onondaga to Buffaloe and Erie, through the counties of Cayuga, Seneca, Ontario, Genessee, and Niagara, a distance of more than 100 miles. They are found also in Montgomery, Madison, St. Lawrence, Oneida, and other counties. At the remarkable sulphureous spring in the town of Phelps, eleven miles north-west of Geneva, they appear like corallines and madrepores. On both sides the Genessee and Tonewanto rivers, they resemble marine shells; while on the east and west banks of Niagara river, they assume, in addition to the already enumerated forms, those that have erroneously been called petrified wasps-nests and honeycombs. In some cases these calcareous petrifications are blended with pyrites; and in others, they are impregnated with a petroleum or bituminous matter, called Seneca oil. On viewing these appearances, the mind recurs to the unascertained time when the oceanic water of the primitive globe rolled over this land, and afterwards, on the emergence of the latter, withdrew to valleys and lower receptacles. The saline waters were thus collected in the places they have since occupied, while the upland became bare and dry on the subsidence of the deluge.

It may therefore be reasonably concluded, that the interior seas, now miscalled lakes, were originally filled with salt water. Their present freshness, on this supposition, is the consequence of the dilution they have undergone, changing them from briny seas to fresh lakes. To understand the subject, let Ontario, Erie, and the upper Lakes of North-America, be compared with the collections of salt water in the other parts of the world. The *Caspian* is naturally salt, and retains that quality because there is no outlet. The waters it receives by the rivers and rains are so nearly balanced by that which goes off by evaporation, that this reservoir has

never burst its boundary. The like observation applies to the *Dead Sea* in Syria. The exhalation from its surface seems to be supplied from the influx of the Jordan, and there has been no sufficient accumulation to force a passage out. The *Mediterranean* has a communication with the Atlantic, and its saltness is preserved by the great supplies it receives through the Herculean Straits near Gibraltar.—The same remark may be made concerning the *Euxine*; though the large and numerous fresh rivers which empty into it, co-operating with its more northern and cold situation, impel the saline part of it through the Thracian Bosphorus and Straits of the Dardanelles into the Archipelago. Should the supply through the Danube, the Dnieper, the Dneister, the Don, and other tributary rivers, be more considerable than the quantity carried off in vapour, the Black Sea must find a discharge into the Egean, and a diminution of its saltness, by dilution with river water, be the consequence. It is accordingly well understood, that the Euxine is undergoing the freshening process, which has long ago been completed in the great American lakes. The *Mexican* presents a case which strongly corroborates this doctrine. Of the two lakes which impart health and convenience to the City of Mexico, the upper one is fresh, and the lower one is salt. The salt is not a muriate, but a carbonate of soda, like that of the Nitrian pools of Egypt. The argument loses none of its force on that account. Two streams which enter the upper lake have washed out the alkali, and carried it down to the lower bason. From this latter, in dry seasons, more water goes off by exhalation than comes in by the current. Extensive shoals are left bare and incrustated with alkaline crystals, which the natives gather and sell. When, from copious rains, this lower lake rises above a certain height, it overflows, as some other ponds are known to do, and finds an occasional outlet.

The inland seas of North-America differ from all those cases, except that of the upper lake of Mexico. They are unlike the Caspian and Judean seas, because these latter have no outlets. They vary from the Mediterranean and Euxine, inasmuch as their supplies are abundant, and the outlets of the American lakes pass along such declivities, and are so rapid and precipitous, that the current always sets vehemently one way and wholly prevents a reflux. And they can scarcely receive a more apt and happy illustration than the Mexican lakes afford. Their original saltness may there-

fore be conceived as having been subjected to incessant dilution, and the freshened waters as having left their reservoirs never to flow back.

Under such circumstances, where the salt water was constantly going forth, and the fresh water running in, it must necessarily have happened, that the former would gradually be exhausted, and its place occupied by the latter. Thus it may be conceived that the ancient saltness of our lakes was lost. When, however, we survey the marine exuviae on their shores, we can with difficulty refuse full credit to these evidences of the former state of things.

When also we reflect that Erie abounds with sturgeons who never visit the ocean, we must conclude that a fish, which used to migrate from sea to river, and from salt to fresh, has gradually been weaned from his marine habits, and become a perfect fresh water animal. The salmon of Ontario is believed by some to be a witness of the same fact. Since the transition, it is said, he has acquired the faculty of living without brine, and become contented with the unsalted water of his native lake.



An instance of conception, wherein a fetus was evolved in each ovarium. In a letter from William B. Smith, Esq. of Jamestown, Prince-Edward County, Virginia, dated September 15, 1809, to Mr. Stanard, editor of one of the Richmond gazettes.

ON the 11th inst. Mr. *Seymour Wright*, of Cumberland County, sent me a note, informing me his negro Diana had just died. Dr. James T. Hubbard saw this woman in her last illness: he believed it to be a singular case, and expressed a desire that Mr. Wright would send for me, with a view of opening her body to ascertain the seat of her complaint. I opened the body in the presence of Mr. Wright, a young man who lives with him, and Mr. Allen Wilson, by first making a longitudinal incision down the middle of the breast to the xyphoid cartilage. I then dissected away the integuments, and raised the sternum. The left lobe of the lungs was very closely attached to the whole surface of the pleura; and in tearing it away, broke into an abscess which discharged nearly a gill of offensive purulent matter. The pericardium seemed unusually distended, and contained a

much larger quantity of that fluid which serves for the purpose of lubricating the heart, than is consistent with health. The convex surface of the diaphragm was considerably inflamed; so much so, that there was an effusion of blood in several places. Continuing the incision down from the scrobiculus cordis to the ossa pubis, leaving the umbilicus to the right, I found all the abdominal viscera in a healthy state, except the spleen and liver, the former of which was very much enlarged, and so much decayed that it would scarcely bear its own weight; weighing, I suppose, upwards of three pounds. The great lobe of the liver considerably enlarged, and the gall-bladder distended with dark glutinous matter; having more the appearance of common tar, than healthy bile. From this circumstance, I suppose there must have been some obstruction in the ductus communis choledochus. The left kidney was much smaller than the right, the cortical part having a very pale and unhealthy appearance.

Examining the contents of the pelvis, I found the uterus pushed forward towards the ossa pubis, uncommonly small and hard. Opening it, with a view of ascertaining its contents, I found the cavity so small, that it was with much difficulty it received the little finger. Being more particular in my examination of the pelvis, I discovered two large substances, which, at first view, I supposed to be indurated fæces; but finding they were suspended by a ligament, and entirely connected with the uterus, except by a slight adhesion to the rectum, and other parts, I did not hesitate to pronounce them diseased ovaria. The left ovarium, which was enlarged beyond the size of a hen's egg, had formed a bed among the intestines, and slightly attached itself to them, particularly to the rectum. The situation of the right ovarum differed very much from the left, being higher in the pelvis, adhering to peritoneum, intestines, &c. its figure very long and irregular.

The uncommon size and figure of these ovaria, induced me to examine them more minutely. I removed them from the pelvis, and obtained leave of Mr. Wright to carry them home. The next day I made an incision with my scalpel in the ovarium which was taken from the right side, and found my knife considerably resisted by some extraneous matter. I then carefully removed the first membrane, or external covering, and found another. After discharging a small quantity of yellow fluid from the second, I discovered

a fœtus, between three and four inches long, perfectly formed, with an umbilical cord joined to a loose vascular and fleshy substance, which I suppose answered the purpose of a placenta. I am well assured that this fleshy production must have been the medium of communication between the parent and child. This placenta (if I may so call it) is about three times the size of the fœtus; the fœtus attached to the upper end of it, and the lower end adhering to the fimbriæ of the fallopian tube. It is very remarkable that although this fœtus is not more than three inches long, the bones of the head, trunk, and extremities, appear to be perfectly ossified, and hard as the bones of an adult, breaking short off before they would give way to the least pressure. The head of this fœtus appears to be twice the size of the trunk and extremities. On opening the abdomen, the liver, which is of a beautiful and delicate yellow, occupies two thirds of the cavity; and the intestines wonderfully convoluted, one third only.

After making every discovery in the right ovarium, I proceeded to the examination of the left; which was much larger than the right, resembling in figure a squirrel's head more than any thing to which I can compare it. I first dissected away the external coat, which I supposed to be a production of the peritonæum, and then passed my knife through the most convex part of it, not suspecting, as in the first instance, another embryo; but, to my great surprise and astonishment, I cut through the spine and ribs of a fœtus. Having opened carefully the ovarium, a fœtus presented itself, eight inches long, with its feet, legs, hands, arms, head, and trunk perfectly formed; and nothing wanting but the abdominal viscera to render it complete in all its parts. It is very surprising that the funis umbilicalis in this fœtus, instead of passing off from the abdomen to the placenta, passed from the anus. This placenta was very different from the other, being much smaller, more firm, and having a more healthy appearance. This, perhaps, is the cause why the last fœtus is larger than the first. I was not able to discover in the first, whether it was male or female; but in the last the male organs of generation were very plainly seen. Three or four vesicles or ova, of different sizes, joined to the internal surface of each diseased ovarium, were discovered. I have the above phenomenon in a glass jar, which may be seen at any time. The woman in whom the above discoveries were made, was about 40 years old; the

mother of six children; the youngest eight years of age. I was informed she has never enjoyed her health since she was delivered of her last child. Before I made these discoveries, I was of opinion the woman might have been saved if properly treated; but if the cause of the disease had been discovered during the life of the patient, it would have been difficult, if not impossible, to afford relief.

How shall we account for the phenomenon? By supposing the male *semen* to be taken into the circulation of the blood, and some of the particles coming in contact with the ova of the ovaria? Or shall we suppose the *semen masculinum* is carried to the ovaria by the fallopian tubes, and there impregnates the ova? These two cases of extra-uterine foetus certainly prove, that conception or impregnation first takes place in the ovaria, and the tubes conduct the impregnated ova into the uterus. In the cases I have mentioned, the fallopian tubes, from some unknown cause, might have been incapable of performing their office, and left the impregnated ova in the ovaria, where they were gradually evolved.

Experiments to determine whether the light emitted by the flash of a gun can enable a bystander to distinguish the features of the person who fires it.

MESSRS. Gineau, Dupuis, Messier, Caussin, Lecuy, Golzart, and Maucler, have made experiments in the imperial college of France, which they reported on the 8th of December, 1808, to the following effect.

They assembled at eight o'clock in the evening of that day, to make a fair trial whether the light from the priming of a gun is capable of distinguishing the features of the shooter. They entered a chamber that was perfectly dark. In this, Mr. Gineau, the younger, flashed the gun several times, pointing the barrel different ways. The spectators kept themselves successively at different distances.

They observed that the flash cast a strong but sooty light, which was so rapidly extinguished that it was impossible to discern the lineaments of face in the person who drew the trigger. It was scarcely possible to distinguish clearly the shape of the head: that of the face could not be recognized.

Professor Gineau having taken the gun, flashed it several times, placing his face in all aspects. The results were in-

variably the same as in the preceding experiments. The transition from a strong light to darkness was so quick, that the eye could see nothing but the light. The face of the shooter was totally invisible.

Being desirous of carrying their conviction still further, they descended into the college yard, and having put out the reverberating lights, the gun was both charged with powder and primed. The younger Gineau then took a station behind the great door, and fired repeatedly in horizontal and inclined directions, to the right and left, and with different presentations of his face both to the light and the spectators; and these varied their distances from three to four, six, and eight metres, and more. The results were in no wise different from the foregoing.

Having repeated and varied the trials until they were satisfied, and without any difference of fact, they came to a solemn conclusion, "that it was impossible for a man, even "under the exertion of his undivided attention, and at a "time when he knows what is about to take place, to be certain, from the effulgence of the priming and loading of a "piece, who the person may be that discharges it near him "or at him."

Colic and Convulsions in early Infancy.

[We are indebted to the polite attention and kindness of Dr. Tillary, President of the Medical Society of the city and county of New-York, for a communication of the following paper, by Joseph Clark, M. D. honorary Fellow of the Royal College of Physicians, Dublin, and Member of the Royal Irish Academy.]

THE frequency and fatal tendency of Colic and Convulsions, during the first three or four months of infancy, have long made them objects of my most serious attention.

After many years deliberation I have been led step by step, and not without many apprehensions, to adopt a practice in these diseases not generally recommended by writers, and which appears to me to have saved many lives. What I have to propose differs so widely from the treatment heretofore pursued, that I doubt whether I should

have had courage to submit my observations to public notice, had not the admirable work of Doctor James Hamilton, on the utility and administration of purgative medicines, made its appearance. This author has illustrated, in a masterly manner, the effects of torpor in the bowels in St. Vitus's dance, and several other diseases hitherto deemed nervous, and has proved the necessity and propriety of such unusual quantities of purgative medicine, that what I have to propose must appear the less marvellous.

The bowel complaints of infants, previous to dentition, have been described in medical works, under the various appellations of "inward fits," costiveness and wind, gripes and green stools. To this catalogue, I am disposed to add in gradation, Bilious Colic and Convulsions. All these appear to me stages of the same disease, and to be more or less connected with the free or restrained discharges of the bowels.

Infants, whose bowels discharge *copiously* four, five, or six times in twenty-four hours, in general sleep a great deal, and appear perfectly at ease, provided they are well supplied with suck. With evacuations much under what I have stated, and especially if not *copious*, a variety of uneasiness will shew itself, which, if neglected, will sooner or later terminate in a smart attack of colic, and this again, if not speedily alleviated, in convulsions.

It is not a little singular that infants, subsisting on food which yields but little solid contents, compared with the food of adults, should require such large and frequent alvine discharges to keep them in health.

I have, on a former occasion,* expressed an opinion, that a large proportion of these discharges is bilious, and I have now to add, after sixteen years close attention to the subject, that in Colic and Convulsions, nothing but a brisk expulsion of the contents of the bowels is likely to afford permanent relief. To effect such expulsion is frequently a task of the utmost difficulty, and requires remedies of the most powerful nature.

When an infant is seized with violent screamings, and refuses to suck, drawing its lower extremities occasionally towards the abdomen, the existence of colic may be safely inferred. On inquiry, the infant may be reported to have

* See Transactions of Royal Irish Academy, Vol. vi. p. 3.

had frequent stools, but on *inspection* they will generally be found scanty, often green.

A dose or two of Castor Oil, or a common purgative Enema, will often remove slight attacks of this nature. It is in general after the failure of such measures, that a physician's advice is required. My practice of late, in urgent cases, is to order twelve grains of calomel, to be rubbed with an equal quantity of refined sugar, and divided into twelve parts. One to be given immediately in a little of the nurse's suck, and in an hour after, a tea-spoonful of castor oil. And alternately calomel and castor oil are directed to be repeated every hour, until the bowels are freely purged, and the infant obtains relief. I have frequently found, that before my visit, on the succeeding day, ten or twelve doses of each have been given, and retained in the stomach. The general effect is many motions, but, on inspection, not more than one or two copious. The relief from pain is often considerable, the convulsions have ceased, and the infant resumed its former habit of sucking. I then direct the powder and oil to be repeated every third or fourth hour, more or less frequently, according to the degree of ease the infant appears to have obtained, and the freedom of its discharges.

On my third visit the bowels have generally discharged a prodigious quantity of green, and apparently *acid* bile, and hence I have denominated the colic *bilious*. In the course of recovery, the quantity of evacuation seldom fails to astonish the attendants, who cannot well comprehend whence it can all be derived. The relief obtained is uniformly proportioned to the quantity discharged.

After the third day a calomel powder is given, perhaps night and morning, until the fourth or fifth, and castor oil now and then, when the infant is uneasy. Where the frequent repetition of oil nauseates the stomach, or where it produces, at the end of twenty-four hours, very little effect, I substitute a desert spoonful of infusion of senna sweetened, and warmed with fifteen or twenty drops of tincture of jalap.

I have lately kept up the purging plan somewhat longer than heretofore, from having seen an infant of six weeks old relapse into convulsions, after continuing two days apparently in perfect health. During the relapse, nothing would pass through the bowels, and it died in great agony. On inspecting the abdominal viscera, the day after its death,

the whole alimentary canal was found empty ; nothing like obstruction any where, but considerable marks of superficial inflammation, more especially on the small intestines.

Although energetic measures, such as I have described, will usually succeed in affording relief at the end of forty-eight or sixty hours ; yet I was once under the necessity of continuing the administration of purgatives for an entire week. For seven days and seven nights, an infant, of six weeks old, had, on an average, a convulsive fit every hour. These began on the eighth day after inoculation for cow-pock. For some days it was difficult to persuade the parents that the convulsions were not produced by the inoculation. In proportion, however, as I found the bowels to resist the operation of physic, in the same degree was I convinced that they were the seat of disease. Notwithstanding all the exertions that I, and a respectable practitioner in consultation, could make by alternating calomel with fluid physic of various kinds, we obtained but scanty evacuations during the week. At length the bowels yielded freely, and in the course of the second week, amply repaid the deficiencies of the first. At the end of two years, I had the satisfaction of learning from this infant's father, that it then enjoyed perfect health. Warm bathing, fomentations to the abdomen, and purgative injections into the rectum, were employed in this case, as in all bad cases of the kind ; but I never fail to inculcate, that these are temporizing measures, intended merely to alleviate pain, and gain time for the operation of physic, which alone deserves our confidence. The cow-pock, in this case, observed a slow, but regular course.

Sympathy for the afflicted, has, on more occasions than once, induced me to consent to a trial of Dalby's carminative, which is a warm opiate, and a favourite medicine with many, in all bowel complaints. Both from theory and practice, I am convinced it is a bad remedy in such cases. After very profuse evacuations from the bowels, it will sometimes produce comfortable effects, by allaying tenesmus, expelling flatulence, &c. &c.

Bilious cholic appears to me to be more prevalent in the summer months, and during one season than another. I certainly met more bad cases in the summer of 1804, than ever occurred to me in one year, and I was so fortunate as not to lose one.

I consider the disease dangerous in proportion to the

torpor of the bowels, and consequent difficulty of forcing a passage through them. My anxiety and exertions uniformly increase as this difficulty appears greater, and it affords me sincere satisfaction to be able to state, that a great majority of such cases terminate favourably.

In the last case of convulsions which occurred, the infant was twelve days old. It had been uneasy from its birth, and had taken castor oil frequently. It awoke from sleep in violent convulsions, and immediate dissolution was so strongly threatened, that the nearest practitioner was sent for, from fear that it might not live until my arrival.

From three o'clock in the afternoon of one day, until noon next day, this infant took ten doses of calomel, each containing a grain and a half, and an equal number of teaspoonfuls of castor oil, with decided relief. In the course of the ensuing three days, nearly as much more purgative medicine was given; so that in four days it consumed half a drachm of calomel, and between three and four ounces of castor oil. It is now well, and continues to be nursed by its mother.

I am aware that prejudices have existed against the practice recommended, and may possibly still exist. To infants in health, I make no doubt such doses of physic would do much mischief; but where torpid bowels cannot be roused into action, and irritating matters expelled by gentle means, what alternative has the most rational physician? Can he stand by and see his patient sink under painful and terrific disease, because a prejudice exists against the exhibition of strong physic in infancy? Every practitioner who has been in the habit of employing mixtures of rhubarb and magnesia, solutions of manna in fennel-water, egg-shells, musk, volatile alkali, opium and blisters, the remedies formerly in use, can be at no loss to form an opinion of the proportion of convulsive cases in early infancy, which recovered under such treatment.

In the beginning of my practice, and so long as I pursued the beaten track, recovery from convulsions in early infancy was a rare occurrence.

Now the case is happily reversed, and death is a rare occurrence.

Rutland-square, May 1st, 1809.

MESSRS. Kimber & Conrad, of Philadelphia, have lately re-published an impression of Dr. Willan's admirable work on *Cutaneous Diseases*. A better selection among British medical publications has seldom been made for the benefit of our American readers. Dr. Willan's work, on this subject, is confessedly the best now extant. When we consider the prevalence and obstinacy of this tribe of diseases, we are assured the medical practitioners of this country will be gratified to possess this volume in a compressed and cheap form, as so many of them were excluded from the advantages of the British edition by the expensive engravings by which it is illustrated and adorned.

New Edition of Burns's Principles of Midwifery.

MR. BURNS has lately transmitted to his friend, Dr. Buchanan, Professor of Midwifery in Columbia College, materials for a new edition of his work, which include a considerable mass of additions, corrections, and other improvements, which will greatly increase the value of that excellent publication. Dr. Buchanan has obligingly submitted to our perusal the body of new matter thus received, which he intends to insert in this edition; and we are gratified to find that the author's latest revisal of his performance will enable him to present it to the American reader in a form substantially extended and improved. The work is now in the press, and will soon be ready for publication.

Course of Popular Lectures on Chemistry.

MR. GRISCOM, whose indefatigable exertions to promote chemical instruction we have often mentioned heretofore, is at this time employed in giving a new course of lectures on select subjects of that science, embracing such parts as are found to be most generally interesting, and adapted to an audience of both sexes. By great diligence, and at considerable expence, this gentleman is now enabled, by his excellent apparatus, to exhibit a course of experimental chemistry, which does much honour to his talents, and affords a high degree of satisfaction to those who engage in the study of this branch of knowledge.

IN addition to the means of instruction which our medical seminaries in this city present to professional students, we mention with pleasure that DR. MOTT, lately returned from Europe, is now laudably engaged in delivering a system of Lectures on Surgery, which, by exhibiting diversified views of that subject, and particularly by offering an account of all the latest improvements by the great European masters of the art, must tend greatly to enhance the advantages which are held out to the industrious student. We are informed that Dr. M. intends to resume his course of lectures, towards the close of the next autumn, under a more extended form.

Rush's Collection of Introductory Lectures.

DR. RUSH intends soon to put to press a second edition of his volume of *Introductory Lectures*, which will contain a number not before published, and particularly two "*On the pleasures of the senses and the mind, with an inquiry into their proximate cause.*"

Officers of the Philadelphia Philosophical Society, elected January 5, 1810.

President—Thomas Jefferson.

Vice-Presidents—Caspar Wistar,
Robert Patterson,
Benjamin S. Barton.

Secretaries—Thomas C. James,
Thomas T. Hewson,
Nathaniel Chapman,
Burgess Allison.

Counsellors for three years—

William White,
Jonathan Williams,
Peter S. Duponceau,
Mahlon Dickerson.

Counsellor for one year, vice James Woodhouse, deceased—
Adam Seybert.

Curators—C. W. Peale,
Robert Hare, jun.
John R. Smith.

Treasurer—John Vaughn.

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